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THE QUARTERLY

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I. A NEW PHASE OF PLANT-LIFE.*

IT is interesting occasionally to look back to the time when Natural History, still in its unscientific stage, consisted of a catalogue of names, diversified, as regards zoology, with apocryphal anecdotes, stilted declamations on the nobility of the horse and the faithfulness of dogs, and with a *fœdissima proluvies* of twaddle about "instinct and reason." Botany was a still more meagre study, relieved merely with notes on the supposed healing virtues of the species described. In those good old days we believed in the existence of sharply-defined genera and orders, not to speak of species. We thought that all beings were duly labelled and pigeon-holed for our more convenient study. Even the time-honoured antithesis of "man and beast" seemed scarcely so glaring and so fundamental as the contrast between animals and vegetables. We could not, of course, refuse to regard the latter as living beings. We saw them developed from a germ, growing by the absorption and assimilation of foreign matter, secreting substances altogether different from the nourishment taken in, reproducing reds similar to that from which they sprung, and finally decaying and being resolved into unorganised compounds. But we regarded them, for all this, as entirely passive. They were held incapable of any other motion than that due to the passing wind. They were supposed to be nourished exclusively upon whatever matter—inorganic or merorganic—came casually in contact with their leaves or rootlets, having at most the somewhat negative power of a *veto*, by refusing to take in anything useless or injurious. Still more decidedly were they considered as insensient, devoid of all feeling,—as incapable of recognising and of reacting upon any contact or impulse from without as are the very stones by the wayside. True, the movements of

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* Insectivorous Plants. By CHARLES DARWIN, F.R.S. London: John Murray.

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the sensitive plant were known, and could not be explained away. But it is remarkable with what ease a fact can be shelved if unconnected, ill-understood, or not in harmony with received opinions. Had the public been informed half a century ago that plants exist decidedly carnivorous—capable of catching and killing insects, and of digesting their remains—scarcely any eminence on the part of the narrator would have saved his statement from being denounced as a fable. Yet in the work which lies before us there is the fullest proof that such is the case. Plants are found in many parts of the globe, our own country included, which depend for subsistence upon the insects which they catch. They are endowed with a sensibility probably more acute than that possessed by any part of the human body, and even discriminate between nutritive and innutritious substances.

It is very interesting that these revelations should be laid before the world by Mr. Darwin. Many authorities—from MM. Quatrefages, Milne-Edwards, and the late Agassiz, down to the Editor of the "Family Herald"—affect to treat our great naturalist as an amateur speculator who has amused himself by commenting upon the information collected by others. His work on "Insectivorous Plants" proclaims him—as has long been known to all candid and competent judges—a careful, patient observer and experimentalist, who, had his attention in early life been directed to physics or to chemistry, might have gathered in those fields of research laurels not less splendid than those he has won in the regions of organic science.

At the same time it must not be supposed that Mr. Darwin is the first and sole observer of every fact detailed in his work. That certain plants catch insects has been long known and commented upon; but Darwin has observed and described, more fully and clearly than any previous author, the mechanism for capture, the circumstances under which it is brought into play, and, above all, has established the capital fact of the true digestion of the prey. Henceforth the existence and the attributes of carnivorous plants are points recognised in botanical science, and must be taken into account by all speculators on the life of plants and on their relations to animals. With characteristic modesty Mr. Darwin sums up his researches by saying—"We see how little has been made out in comparison with what remains unexplained and unknown."

The sun-dew (*Drosera rotundifolia*) is a plant not uncommon on heaths, and, though curious, is not remarkable either for beauty or utility to man. Its leaves are covered with so-called tentacles,—hair-like filaments, generally standing upwards, and each carrying on its summit a gland which may be roughly compared to the head of a pin. These glands are "each surrounded by large drops of extremely viscid secretion, which, glittering in the sun, have given rise to the plant's poetical name of the sun-dew." This secretion, acting to some extent like bird-lime, is the primary agent in capturing insects. "When an insect alights on the central disc it is instantly entangled by the viscid secretion, and the surrounding tentacles after a time begin to bend, and ultimately clasp it on all sides. Insects are generally killed, according to Dr. Nitschke, in about a quarter of an hour, owing to their tracheæ being closed by the secretion. If an insect adheres to only a few of the glands of the exterior tentacles, these soon become inflected, and carry their prey to the tentacles next succeeding them inwards; these then bend inwards, and so onwards until the insect is ultimately carried, by a curious sort of rolling movement, to the centre of the leaf. Then, after an interval, the tentacles on all sides become inflected, and bathe their prey with their secretion in the same manner as if the insect had first alighted on the central disc. It is surprising how minute an insect suffices to cause this action: for instance, I have seen one of the smallest species of gnats (*Culex*) which had just settled with its excessively delicate feet on the glands of the outermost tentacles, and these were already beginning to curve inwards, although not a single gland had as yet touched the body of the insect."

To measure, as it were, the sensitiveness of the glands, and ascertain what were the smallest bodies capable of inducing motion in the tentacles, a number of delicate experiments were instituted. It was found that little bits of human hair, measuring only 8-1000ths of an inch in length and weighing only 1-78740th of a grain, caused the tentacles to curve inwards. The author remarks that a bit of hair 1-50th of an inch in length, and therefore much larger than those used in the above experiments, was not perceived when placed on his tongue. "It is extremely doubtful," he adds, "whether any nerve in the human body, even if in an inflamed condition, would be in any way affected by such a particle supported in a dense fluid, and slowly brought in contact with the nerve. Yet the cells of the glands of *Drosera* are thus excited to transmit a motor impulse to a distant point, inducing movement. It appears to me that hardly any more remarkable fact than this has been observed in the vegetable kingdom." But this extreme sensibility is not promiscuous in its nature. It reacts to continuous pressure to the wonderful extent just recited, but a momentary touch, even several times repeated, produces no inflection. "On one occasion forty-five glands on eleven leaves were touched once, twice, or even thrice, with a needle or stiff bristle. This was done as quickly as possible, and with force sufficient to bend the tentacles; but only six of them became inflected—three plainly and three in a slight degree." Yet these tentacles, so indifferent to a passing touch, were quickly deflected if bits of meat were laid upon them, and were thus found to be in a normal, healthy condition. This comparative insensibility to touch, the author very justly argues, must be of advantage to the plant, since in windy weather its leaves must be occasionally brushed by the tall blades of grass growing near. "It would be a great evil if the tentacles were thus brought into action, for the act of re-expansion takes a considerable time, and until the tentacles are re-expanded they cannot catch prey." But prolonged pressure—even from utterly innutritious matter, such as glass, cinder, gold-leaf, cork, &c.—produces inflection. This is a serious consideration for teleologists, as furnishing proof that the plant is not perfectly adapted to the circumstances in which it is placed. "If a bit of dry moss, peat, or other rubbish, is blown on to the disk, as often happens, the tentacles clasp it in a useless manner. They soon, however, discover their mistake, and release such innutritious matter."

Drops of water falling upon the leaf—whether as natural rain or artificially sprinkled—produce no effect. This is the more remarkable as small rain-drops often adhere to the viscid secretion of the plant, and must occasion a pressure vastly greater than that of the bits of hair above mentioned. It is obvious that to be affected by rain-fall would be highly inconvenient to the plant.

The success of the sun-dew in capturing prey is great. "On one plant all six leaves had caught their prey, and on several plants very many leaves had each caught more than a single insect. On one large leaf I found the remains of thirteen distinct insects." Hence it would seem that the snares of the sun-dew are quite as efficient as a spider's web. As to the species captured it appears that flies (Diptera) are the commonest victims. "The largest kind which I have seen caught was a small butterfly (*Cœnonympha pamphilus*), but the Rev. H. M. Wilkinson informs me that he found a large living dragon-fly with its body firmly held by two leaves." A kindred species, *Drosera filiformis,* very abundant in New Jersey, catches an extraordinary number of small and large insects—even great flies of the genus *Asilus,* moths, and butterflies.

The question now arises whether insects alight on the leaves by mere chance, as a casual resting-place, or whether there is some object of special attraction which serves as a bait for the trap. On this point Mr. Darwin suspends judgment. "I suspect," says he, "from the number of insects caught by the English species of *Drosera*, and from what I have observed with some exotic species kept in my greenhouse, that the odour is attractive." We should think that insects may be tempted to settle on the plant by the dew-like appearance of the secretion on the glands. Flies especially are well known to be "thirsty souls," and rarely lose an opportunity of drinking. Butterflies are also very fond of sipping moisture, as from the edges of shallow forest-pools.

But we must now enquire what becomes of the insects thus caught and killed? There are two possibilities: their bodies may pass into putrefaction, the plant being nourished by the liquid and gaseous products, or they may undergo a veritable process of digestion, as if they had been introduced into the stomach of an animal. Mr. Darwin has clearly shown that the secretions of the *Drosera* act on albumenoid compounds exactly as does the gastric juice of the Mammalia, the digested matter being afterwards absorbed. "This fact," he very justly remarks, "is a wonderful one in the physiology of plants. It is well known that the digestive process in animals requires the presence of an acid in company with the pepsin secreted by the stomach. If this acid is neutralised by an alkali, digestion is arrested, and goes on again if the alkali is again saturated by a suitable proportion of an acid. The very same results were obtained with the *Drosera*. Small cubes of albumen were given to the plant, and began to be dissolved in the ordinary manner. Small quantities of alkaline solutions were then added, so as to neutralise the acidity of the juices secreted by the plant. Immediately the action was brought to a stand-still, and the albumen was no longer attacked. If, now, traces of weak hydrochloric acid were added, so as to restore the secretion to its normal condition, the process of digestion was at once resumed. Within forty-eight hours from the time the acid was given the four cubes were not only completely dissolved, but much of the liquefied albumen was absorbed." Small portions of the ferment—removed from the plant with due precaution and placed in a glass—were found able to dissolve fragments of coagulated albumen. In all the numerous experiments made on the digestion of cubes of albumen "the angles and edges were invariably first rounded off." This, according to Schiff ("Lecons Phys, de la Digestion," vol. ii., 1867, p. 149), is "a special characteristic of the digestion of albumen by the gastric juice of animals." When, on the contrary, such cubes are dissolved by chemical action, solution goes on over the whole surface in contact with the solvent.

Another very important point is that the secretion of the *Drosera*—like the gastric juice of the higher animals—possesses a decidedly antiseptic property, and, whilst effecting digestion, checks putrefaction. This interesting fact is proved by the following experiments:—"During very warm weather I placed close together two equal-sized bits of raw meat—one on a leaf of *Drosera*, and the other surrounded by wet moss. They were thus left for forty-eight hours, and then examined. The bit on the moss swarmed with Infusoria, and was so much decayed that the transverse striæ on the muscular fibres could no longer be clearly distinguished; whilst the bit on the leaf, which was bathed by the secretion, was free from Infusoria, and its striæ were perfectly distinct in the central and undissolved portion. In like manner small cubes of albumen and cheese, placed on wet moss, became threaded with filaments of mould, and had their surfaces slightly discoloured and disintegrated; whilst those on the leaves of the *Drosera* remained clean, the albumen being converted into a transparent fluid."

Among the substances thus dissolved and digested by the leaves of the *Drosera* may be enumerated roast meat, pure fibrin, areolar tissue, fragments of bone, and pure phosphate of lime. The latter, however, and also raw meat, are too powerful stimulants, and, except in very minute doses, injure or kill the leaves. Fat is not digested. Gelatine and isinglass have a comparatively feeble exciting action on the plant. "This," remarks Mr. Darwin, "is an interesting fact, as it is well known that gelatine by itself has little power of nourishing animals." The secretion of the *Drosera* quickly dissolves casein in the condition in which it is found naturally existing in milk. But the pure chemically-prepared casein is scarcely, if at all, attacked. Such casein we learn on high authority is almost entirely insoluble in the gastric juice of animals. This is another evidence in favour of the identity of the secretion of the *Drosera* with gastric juice, since both act so differently on the fresh casein of milk and on the dry matter prepared by chemists.

Cheese was digested only to a slight extent, and proved injurious to the leaves. Legumen appears to be digested and absorbed. The pollen-granules of plants are penetrated by the secretion, and the contents partially digested. Gluten is partially acted on, but—like raw meat, phosphate of lime, or even albumen in large pieces—it is too powerful a stimulant. But gluten freed from starch, by steeping in very dilute hydrochloric acid, was readily digested, and was not found to injure the glands of the leaves in any marked degree.

Among the substances not acted upon by the secretion of *Drosera* are hair, bits of finger-nails, quills of feathers, mucin, urea, chitin (the chief constituent of the outward skeleton of insects), chlorophyll, cellulose, fats, oils, starch, sugar, gum, dilute alcohol, and vegetable extracts free from albumen. Here is further proof of the identity of the *Droseræ* ferment with gastric juice, since none of the substances just enumerated—as far as is known—are digested by the gastric juice of animals, though certain of them are acted upon by the other secretions of the intestinal canal.

The *Drosera* may be termed an omnivorous plant; for though its principal food, doubtless, consists of insects, it is also capable of deriving nourishment from the pollen of plants, which, doubtless is occasionally deposited by the wind upon its leaves. Even seeds, though not absolutely dissolved, are attacked by the secretion, and some principle is doubtless extracted out of them. It is at least certain that seeds placed upon the leaves of the *Drosera* produce inflection of the tentacles, and are subsequently found to have been injured. Of seven cabbage seeds thus treated three only were found capable of germinating, and of the three seedlings one speedily perished. Very similar results were obtained with the seeds of radish and cress. On the other hand, seeds of black mustard, celery, caraway, and wheat were found not to excite the plant more than inorganic objects. But the identity, or at least the close similarity, existing between the gastric juice of animals and the secretion of the *Drosera*,—two complex liquids subserving the same function, though elaborated by organisms so remote from each other,—though it may justly be styled "a new and wonderful fact in physiology," is not by any means the only marvel which the study of the *Drosera* reveals. We have already seen that a drop of water, falling upon the leaves of the plant, entirely fails in causing any inflection of the tentacles; but the case is very different with fluids containing nitrogen, whether in the shape of organic compounds or of ammoniacal salts. Nine salts of ammonia which were applied to the plant, in the state of weak solution, all caused the inflection of the tentacles, and frequently also of the blade of the leaf. Of all the salts tried the phosphate is by far the most powerful stimulant. The 1-3840th part of a grain placed on the glands of the disc, so as to act indirectly upon the outer tentacles, produced this result. But this is far from being the limit. If the solution was applied for a few seconds directly to the gland of an outer tentacle, 1-153,600th of a grain was sufficient, whilst if the leaf was entirely immersed—allowing time for every gland to absorb all that it can—inflection was produced by the almost inconceivably small quantity of 1-19,760,000th of a grain. As, moreover, the phosphate of ammonia in question contains 35 per cent of crystalline water, the really efficient active matter in the solution would be, in round numbers, the 1-30,000,000th of a grain! This is a degree of sensitiveness far surpassing that of any method of analysis, with the exception of the spectroscope. Dissolve 1 grain of phosphate of ammonia in a 31-gallon cask full of water. Of this solution take ½ a drachm. The most sensitive reagents, in the hands of the most skilful chemist, will fail to show the presence of the salt. Yet the *Drosera*—a plant without any specialised nervous system, insensient in common belief as the clods of the valley—detects it at once. We see no reason here for suspecting any error. The salt, Mr. Darwin informs us, "was in some cases weighed for me by a chemist in an excellent balance." Now it is scarcely necessary to observe that balances of modern construction, and chemists of modern training, are required to weigh with accuracy much smaller amounts than 1 grain. The quantities of water in which this 1-grain dose was to be dissolved were many times measured with great care. The experiments were repeated during several years by Mr. Darwin himself and his two sons, who were at first as incredulous as himself; and simultaneous trials were made with other leaves, immersed in still weaker solutions and in pure water, by way of control. "I hope," adds the distinguished author, "that some one may hereafter be induced to repeat my experiments: in this case he should select young and vigorous leaves, with the glands surrounded by abundant secretion. The leaves should be carefully cut off and laid gently in watch-glasses, and a measured quantity of the solution and of water poured over each. The water used must be as absolutely pure as it can be made. It is to be especially observed that the experiments with the weaker solutions ought to be tried after several days of very warm weather. Those with the weakest solutions should be made on plants which have been kept for a considerable time in a warm greenhouse or cool hothouse; but this is by no means necessary for trials with solutions of moderate strength."

It may, perhaps, allay the scepticism, especially of "anti-Darwinians," who may read this passage, if they reflect that this high sensitiveness of the *Drosera* is merely in harmony with other well-known facts. Professor Donders and Dr. de Ruyter, of Utrecht, find, from their experiments, that less than one-millionth of a grain of sulphate of atropia, in an extremely diluted state, if applied directly to the iris of a dog, paralyses the muscles of this organ. The odorous particles which, floating in the air, are detected by the olfactory nerves, must be vastly smaller than the minutest dose of phosphate of ammonia recognised by the *Drosera*.

With a view to ascertain the seat and the nature of the sensitiveness observed in this remarkable plant, a series of careful experiments have been instituted with a variety of agents. Some of them produced a poisonous influence, but others which have a powerful action upon the nervous system of animals, produce here no effect. Hence it is concluded that—"the extreme sensibility of the glands, and their power of transmitting an influence to other parts of the leaf, causing movement, a modified secretion or aggregation does not depend on the presence of a diffused element allied to nerve tissue." This was shown by a variety of interesting experiments. The leaves of the *Drosera* were brought in contact with a variety of narcotics. Several of these, "which act powerfully upon the nervous system of animals, produce no effect upon *Drosera*." We may in particular mention the poison of the cobra, so well-known for its rapid and deadly action upon the nerve-centres of animals. But prolonged immersion in this poison, far from checking, appears rather to stimulate "the spontaneous movements of the protoplasm in the cells of the tentacles." Hence, then, we have, in this plant, sensation and the transmission of impulse not merely without demonstrable nerves, but apparently without nerve-tissue, and in like manner we have movement without muscular fibre! Concerning the mechanism of these movements, and the nature of the impulse, our knowledge is still very rudimentary. Yet the mere fact that such movements and such impulses exist, is surely most significant.

But the various species of *Drosera* are far from being the sole insect-catching and flesh-digesting vegetables. In North Carolina grows the *Dionæa muscicapa*, "Venus's fly-trap," a carnivorous plant still more highly specialised. This plant does not entangle its prey by the aid of an ever-ready glutinous secretion, but closes upon it like a spring trap. The leaf has two lobes standing at the inclination of rather less than a right angle to each other. "Three minute pointed processes or filaments, placed triangularly, project from the upper surfaces of both, and are remarkable from their extreme sensitiveness to a touch. The margins of the leaf are prolonged into sharp, rigid projections, which I will call spikes, into each of which a bundle of spiral vessels enters. The spikes stand in such a position that when the lobes close they interlock like the teeth of a rat-trap. The midrib of the leaf, on the lower side, is strongly developed and prominent." The leaf, unlike that of *Drosera*, is sensitive to the momentary touch of a solid body, but is also unaffected by drops of water or currents of air. As soon as one of the filaments is touched, the lobes close with remarkable quickness, and the marginal spikes interlace. So firmly are the lobes thus pressed together, that if any large insect has been caught a corresponding projection on the outside of the leaf is distinctly visible. When closed, they resist reopening with an astonishing force, and are generally ruptured before yielding. But if pulled asunder without being torn, they close again, according to the statement of Dr. Canby, "with quite a loud flap." Though this plant makes no use of any viscid secretion to capture its prey, yet when an insect is once enclosed a fluid is poured out analogous to that secreted by the *Drosera*, but more decidedly acid, and a process of digestion and absorption sets in. If, however, a leaf has closed over any non-nitrogenous, and consequently innutritious, matter, such as wood, cork, moss, or paper, the leaf remains quite dry, and soon re-expands. Over a nitrogenous body, on the contrary, the leaf remains closed for many days, and when it re-opens it is still torpid, and never acts again, or at most only after a considerable lapse of time. The insects caught differ from those generally captured by the *Drosera*. This latter plant, aided by its bird-lime, is able to secure the tiniest and most rapidly-flying insects. Out of fourteen leaves of the *Dionæa* containing captured insects, three had caught ants, five

Elaters, two *Chrysomelas*, one a *Curculio*, one a thick and broad spider, one a *Scolopendra*, and one a fly. Thus the plant may be said to prey upon apterous species, or upon such as, though winged, cannot instantly take flight when the lobes of the leaf close. Very small insects may escape between the spikes, and some very strong species may succeed in forcing their way out. This was once observed by Mrs. Treat in the case of a species of rosechafer (*Macrodactylus subspinosus*). As regards the range of its digestive powers, *Dionæa* corresponds very closely with *Drosera*. As regards the movement of the leaves, we cannot omit to mention Dr. Burdon Sanderson's beautiful discovery that there is a normal electric current in the blade of the leaf and in its foot-stalk; when the leaf is irritated, the current is disturbed in the same manner as takes place during the contraction of the muscles of an animal. It is noteworthy that *Dionæa* is a less prosperous member of the plant world than is *Drosera*. Whilst the latter has been developed into about 100 species ranging in the Eastern Continent from the Arctic regions to South Africa, India, and Australia, and in the Western from Canada tc Tierra del Fuego, *Dionæa* forms only a single species, limited to one district in Carolina.

Our attention is next drawn to the *Aldrovandas*, small aquatic plants allied to the Droseraceæ, and capable of securing and preying upon living creatures. Here also we find the secretion of a true digestive fluid and subsequent absorption of the matter thus digested. But in these species we find processes which appear also to absorb excrementitious and putrescent animal matter. "If this view is correct," says Mr. Darwin, "we have the remarkable case of different parts of the same leaf serving for very different purposes, one part for true digestion, and another for the absorption of decayed animal matter. We can thus also understand how, by the gradual loss of either power, a plant might be gradually adapted for the one function to the exclusion of the other." In the genera *Utricularia*, *Genlisea*, and their allies, we find, accordingly, that though animals are entrapped, they are not digested, but allowed to pass into putrefaction, upon the products of which the plant is nourished. Mr. Darwin's final summary may be usefully quoted in full:—

"Ordinary plants of the higher classes procure the requisite inorganic elements from the soil by means of their roots, and absorb carbonic acid from the atmosphere by means of their leaves and stems. But we have seen that there is a class of plants which digest and afterwards absorb animal matter, namely all the Droseraceæ, Pinguicula, and, as discovered by Dr. Hooker, Nepenthes, and to this class other species will almost certainly soon be added. There is a second class of plants which, as we have just seen, cannot digest but absorb the products of the decay of the animals which they capture, namely, *Utricularia* and its close allies; and from the excellent observations of Dr. Mellichamp and Dr. Canby there can scarcely be a doubt that *Sarracenia* and *Darlingtonia* may be added to this class, though the fact can hardly be considered as fully proved. There is a third class of plants which feed, as is now generally admitted, upon the products of the decay of vegetable matter, such as the bird's-nest orchis (*Neottia*). Lastly there is the well-known fourth class of parasites (such as the mistletoe), which are nourished by the juices of living plants. Most, however, of the plants belonging to these four classes obtain part of their carbon, like ordinary species, from the atmosphere."

A writer unknown, criticising Mr. Darwin's work in a daily paper, winds up an otherwise not unfair notice with the following strange comments:—

"Mr. Darwin forbears to connect his discoveries in this direction with his general theory, and we have no wish to challenge his adherents on the subject. But the existence of these insect-eating plants does seem to us to militate against the theory of infinitesimal changes, each perpetuated by its beneficial action on the life of the species. For it is only when the tentacles and filaments and valves which render seizure possible have become so far complete as to capture at least a few insects, and when the powers of digestion have been acquired, that the plant can benefit by these exceptional developments. And by what influences were the successive changes in this direction sustained and increased, while of no use, till they reached the point of high elaboration at which they become useful, except by that very intellectual providence and controlling purpose which it is the paramount object of Darwinism to exclude?"

Now by what exact stages the *Dionæa* and the *Drosera* have reached their present stage of development it would be utterly premature to pronounce. But we see, in the case of the *Drosera*, that the adaptation is imperfect—a feature intelligible if it has been attained by a process of evolution, but scarcely conceivable on the hypothesis of special creation. Whoever reads Mr. Darwin's work will find that many plants are able to destroy insects without the somewhat elaborate arrangements which we recognise in the *Drosera* and *Dionæa*—that some plants derive no benefit at all from the victims, and that others, advancing a step higher, utilise the products of their decaying remains as manure. So that plants can benefit by these "exceptional developments" *before* "powers of digestion have been acquired." Nor, therefore, have any mysterious "influences" been required to sustain and increase the successive changes in this direction, since we see that they may have been of use long before their present point of high elaboration was reached. The concluding sentence is worthy of a Caccini. The "paramount object of Darwinism"—or rather we might say the sole object—is to elucidate the origin and existence of species. The uses or abuses to which it may be put—whether by theologians or anti-theologians—are independent issues with which the naturalist, as such, is not concerned.

What reply can the advocates of Specialism furnish if challenged to point out some good reason why the *Drosera* and the *Dionæa* should have been chosen for endowment with the powers of catching and digesting animal prey? If the object were to thin a redundant insect population, surely some more common plant would have better answered the purpose than one which, like the *Dionæa*, appears to be dying out. On this subject the reviewer quoted remarks:—"Suggesting to us rather their character as monstrosities, exceptions to the general tendency of nature, which must naturally be rare." This notion is on a level with the theory that fossil remains were *lusus naturæ*. "Monstrosities,"—as applied to an entire species,—"general tendency of Nature," are phrases which betray an utterly unscientific, or rather anti-scientific, vein of thought.

II. VEGETARIANISM: THE GREAT DIETETIC REFORM.

A GARRULOUS old man, who long ago came under our observation, was one day relating to a group of haymakers his own early adventures. One of the audience quietly chalked down on a tree-trunk the number of years which this rural Münchausen—according to his own version—had passed in different parts of the world, and at last greatly amused the audience and confounded the speaker by exclaiming that the total reached the very patriarchal figure of three hundred and eighty-four years. In like manner it should be a consolation for us to know that, plentiful as evil is in the world, it can be accounted for many times over. Different bodies of world-betterers have traced it all to its sources, and can point out the way to its abolition, if mankind would only follow. The teetotaller assures us that if we will but consent to abstain altogether from alcoholic beverages, at least 60 per cent of the existing poverty, vice, crime, disease, and so forth, will forthwith cease to exist. The crusaders against the use of tobacco trace a more modest portion, say 10 per cent, to the use of the American weed. Enthusiastic educationists contend that full one-half of the above-mentioned evils spring either from ignorance or from knowledge not instilled in the authorised place or by the authorised persons. A certain body of sanitary reformers contend that if we would only convert half England into an irrigation-farm a long step towards the golden age would be taken, and another large percentage of poverty, disease, and death would disappear. In Dr. Richardson's model city, Hygeia, the death-rate is to fall to five per thousand, and we presume a corresponding decrease of sickness and debility is to be insured. Political economists are disposed to attribute a very large portion of existing evils to the neglect of saving, to "unorganised" charity, and to the giving of out-door relief. The Malthusians ascribe a preponderating amount of whatever is undesirable to the too rapid increase of population. The "Shrieking Sisterhood" maintain, *con strepitu*, that the decay of empires, the want of public and private morality, and the low standard of intelligence are due to the exclusion of women from the franchise, from Parliament, and from the learned professions. Whilst we listen in amazement to these conflicting oracles, who jointly, if we may believe their statements, account for much more than 100 per cent of existing evils, we are saluted by a fresh voice. A band of philanthropists boldly tell us that the world is to be regenerated by a change of diet; that our woes—moral, physical, and economical—spring from the consumption of animal food; the very taste for alcoholic drinks, they declare, would leave us if we would but abjure beef and mutton and live upon the vegetable productions of the earth. It is difficult to calculate the extent of the changes which this reform would involve. We may form some idea of a country under the operation of the Maine Law or the Permissive Bill; but it is scarcely possible to imagine the trades which would be affected, and the interests which would suffer from a general—voluntary or compulsory—abjuration of animal food.

It may not be uninteresting to examine the various hopes of improvement and benefit held out by vegetarian advocates, and consider in how far they are likely to be realised, and also to consider some of the arguments advanced to prove that man, in making use of animal food, violates the "laws" of his nature.

Let us first turn to the economical phase of the question. Vegetarians contend that on their system an individual or a family may be supported more cheaply than on a mixed diet, and that their "reform" would enable a given country to maintain in comfort a larger population than is now practicable. The latter and more important of these propositions we will now examine.

It may be granted that 100 acres of fair lowland soil will support a greater amount of human life if planted with wheat, potatoes, and other crops directly consumed by man, than if it were laid out in permanent pasture or set with Italian rye-grass, with mangolds, Swedes, and vegetables intended for the food of cattle. It is plain that a certain amount of waste must take place, and that all the nutritious matter taken up by sheep and oxen from their pasturage does not re-appear, weight for weight, in the butcher's shop, in the form of mutton and beef. But unfortunately there is in England—and more or less in all countries—abundance of land which, from the shallowness and nature of the soil and the great elevation above the sea-level, is unfit for tillage. A great part of this land, however, yields excellent pasturage, and is at present utilised for grazing. Under a general and consistent vegetarian *regime* these lands would be thrown entirely out of use, and must cease to yield their quota to the food-market.

The woods and the moorlands produce, on our present system, a certain amount of human food in the shape of game. This amount may not be very great,* but it is still an item in the list of production, and if animal diet be proscribed it must entirely disappear.

A far more serious consideration is the vast stock of nourishment for which we are now indebted to the world of waters, and which we must renounce if vegetarianism is to prevail. Fishes, crustaceans, mollusca, certain amphibians, sea-birds, and even aquatic mammalia, enter more or less abundantly into the diet of every maritime population. Of the total amount of food thus obtained it is difficult to form an adequate conception. According to Simmonds† the total yearly produce of the cod-fisheries of the North-American coast alone amounts to 1,500,000 tons of fresh fish. Let us suppose that only one-half of this is fit to be eaten, and we have 750,000 tons of food which vegetarians ask us to reject. Unless they can show us how to make the ocean yield us an equivalent amount of vegetable matter, we can only reply "*Non possumus.*"

Hence, then, we fear that the economical advantages which vegetarianism seems to offer, if we look merely to rich arable soils, will fade away if we take the whole of the globe—both land and water—into our consideration.

Very probably vegetarian advocates will take exception to that portion of our argument which refers to the disuse of mountain grazing-ground. They will urge that, though they reject flesh, they admit milk, butter, and cheese as a part of their diet, and that these pasture-lands will therefore be made available as heretofore. We will not here, in reply, stay to discuss their consistency in thus sanctioning the use of matters purely animal—a concession, by the way, which improves their cookery much more than their logic. The farmer, at present, finds it profitable to keep cattle, and to dispose of the milk, cheese, and butter at certain prices; but in doing all this he works under conditions which in a vegetarian country would not exist. When a cow ceases to give milk in remunerative quantity he fattens her for the market. The surplus bulls he can at present utilise as veal in their early days, or as oxen when fully grown; but under the vegetarian system he must either let the aged cows and the surplus bulls live as pensioners for the rest of their lives, or he must—if permitted—destroy them without finding a market for their flesh. In either case he will sustain a considerable loss. The consequence will be that he must ask for milk, butter, and cheese prices practically prohibitive, and if he cannot obtain these he must abandon cattle-keeping, and allow the grazing-lands to go out of use.

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* About 70,000,000 rabbits are consumed annually in France.

† Waste Products and Undeveloped Substances, p. 156.

But the derangement which would flow from the general adoption of vegetarianism would not be restricted to articles of diet. There are many well-known substances of great economical and industrial importance which we obtain either directly, by the slaughter of animals, or under circumstances which would be greatly modified if such slaughter were prohibited. Thus train and cod-liver oils, tallow, hides, and hair could scarcely be procured in a consistenily vegetarian country. Even wool would undoubtedly become scarce and dear if the market for mutton were closed.

The refuse of the fisheries is rising into importance as a manure fully equal to Peruvian guano. But if fish might no longer be captured the supply of this fertiliser would be cut off, unless, indeed, the destruction of animal life for purposes other than food received an exceptional sanction. Even then the cost of the raw material would be greatly enhanced. There is, finally, another light, in which the abandonment of the fisheries would have to be regarded. In the past and the present they have afforded maintenance to a hardy maritime population, and have been justly considered a most valuable school for seamen. Surely it would be a mistake to close this school, and to put an end to the trade on which that population depends.

Finding thus little prospect of economical advantage from the general adoption of the vegetarian system, let us turn to a yet more important sphere, and ask—Would the rejection of animal diet render man morally better? We have here the opportunity to apply the "method of variations." If flesh-eating is the main cause—or even one of the main causes—of crime and vice, we shall assuredly find the criminality of a nation increase in the same proportion as the amount of animal food it consumes, other things being equal. Now we are bound to admit that other things in this case are far from equal. The question is complicated by differences of race, religion, and government, to say nothing of minor causes. Still a careful examination can scarcely fail to disclose the moral influences of diet, if such exist. Turning to the extreme north we find the Greenlanders and Esquimaux, a race who, from the necessities of their position, are purely carnivorous. In fact—with the exception of a little scurvy grass, used more as medicine than as diet—they can scarcely be said to partake of any vegetable matter at all. Now if an animal diet produces the effects alleged by vegetarian advocates, we ought to find these people the most vicious—and especially the most bloodthirsty—human beings on the earth. We must further remember that, except in the few places where they have come in contact with European missionaries, no influences have been at work which might correct the supposed evil tendencies of an exclusively animal diet. Yet the facts are quite otherwise. The "Skraeling" is doubtless a low, dirty savage, but he is by no means remarkably ferocious or bloodthirsty, nor even very pugnacious. Europeans wrecked on the inhospitable shores inhabited by these tribes have very rarely met with ill-treatment. Where the Esquimaux come in collision with the Red-skins* of North America they rarely attempt to defend themselves against the aggressions and outrages of the latter, but quietly withdraw farther and farther to the north.

Let us next look at the Red-skins themselves. They are by no means a mild race, and may even deserve to be called ferocious and sanguinary. That they are carnivorous we of course admit; but they are far less exclusively carnivorous than the Esquimaux, since a part at least of their diet consists of the wild fruits of the country and of maize. Here, then, we see among two savage races the one which is purely flesh-eating far less pugnacious and bloodthirsty than the one which enjoys a mixed diet.

Going further southwards we find the ancient Mexican empire infamous for its human sacrifices and other cruelties exercised under the name of religion. Now the Mexicans of old, if not vegetarians, were far from being exclusively flesh-eaters. Agriculture and horticulture flourished among them; and we must remember that a settled people who have no domestic animals such as the ox, sheep, or pig, will always be compelled to subsist mainly upon vegetable produce. Here, then, we have another people less carnivorous than the native tribes on their northern frontier, but certainly not more gentle and merciful. In South America we have a very instructive instance. In the vast plains of La Plata we find the Guachos, one of the most purely carnivorous peoples on the globe, and certainly reckless of human life and ready with the dagger; but across the Andes we find, in Chili, men of the same race, and placed under very similar institutions, but using an abundance of vegetable food. Now the Chilian peasant is every whit as vindictive and as murderous as his neighbours east of the Andes. Crossing the Atlantic we glance at Dahomey and Ashanti, countries literally reeking with wanton bloodshed; yet animal matter does not form the whole, or even the preponderating part of their diet. The Malay is well known for his vindictive disposition and his proneness to assassination; but he comes in the selection of his food far nearer the vegetarian standard than many milder races. It is a significant fact, to which we may have to revert, that among savages cannibalism is most prevalent where animal food is sparingly accessible. In Europe we find the proportion of flesh consumed varies greatly. England, Holland, France, Germany, and Scandinavia are more carnivorous than Spain or Italy. But in England, with all its faults, respect for human life is far greater and assassination far less frequent than in Spain.

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* This term is inelegant, but we use it under compulsion. To call the aborigines of America "Indians" is a blunder which is confusing, and for which there is no excuse.

Hence we must conclude that a mainly or partly animal diet does not in man necessarily produce ferocity and bloodthirstiness any more than a vegetable regimen involves gentleness and humanity. It is not even proved that the butcher, the poulterer, the fisherman, the sportsman, and the angler are at all more cruel and vindictive in their dealings with their fellow-men than are persons who have never taken animal life. We are indeed, on the opposite side, reminded of some vegetarians who were noted for their extreme benevolence. Of this class we may take Shelley as the type; but the question arises—Was Shelley humane and benevolent from his vegetarian habits, or was he not rather a vegetarian from his excessive benevolence? We hold the latter view.

Vegetarians, however, try to make out their case by referring to the lower animals, and enlarging on the ferocity of the Carnivora and the mildness of the Herbivora. Unfortunately this alleged mildness is a myth. The elephant is a vegetarian "pure and simple;" yet a "rogue" elephant will, without the smallest provocation or necessity, attack and kill men, horses, or other animals whom he meets. One at least of the African species of rhinoceros is given to wanton aggression, and is more dreaded by travellers than the lion. Among the ruminants it is hard to name a species which, if gifted with the needful strength, will not attack man. The malevolence of the common bull is well known. The common buffalo of Southern Europe and Asia is never to be trusted. The wild cattle of Lyme and Chillingham, and those of Lithuania, take the offensive whenever the opportunity occurs. As for the Cape buffalo few beasts of prey can surpass him in ferocity. He-goats and rams not unfrequently attack passers-by. The red deer, at certain seasons of the year, cannot be approached with safety. Several of the larger kinds of antelopes are of an aggressive disposition. The larger baboons, though feeding upon fruits, nuts, and vegetable matter, generally are decidedly ferocious. Very similar is the case with vegetarian birds. An old male ostrich is decidedly to be shunned. The common cock, the turkey cock, and the gander are quarrelsome and aggressive creatures, and will occasionally indulge in wanton attacks. An "auerhahn" (*Tetrao urogallus*) once flew at us with such pertinacity that we were able to take him prisoner. From these instances—which, if needful, might be greatly multiplied—we conclude that diet has very little to do in determining the ferocity or the mildness of an animal. Both herbivorous and carnivorous beasts, if attacked or annoyed, will resist with equal determination. If anyone doubts this let him offend an elephant, and "make a note" of the result.

Vegetarians advance yet one more argument. They tell us that a young tiger brought up in captivity, and fed on milk, bread, and cooked meat, will be "mild and gentle," but that if by accident it takes blood or raw flesh its "ferocity" is immediately developed. We admit the facts, but we demur to the interpretation. So long as the tiger has never tasted blood it remains in ignorance that human beings are articles adapted to its taste. As soon as it has had such a meal its sense of smell informs it that men consist of matter similar to what it has already eaten with relish. But where is the "ferocity" in this? Is there really more ferocity in eating animals than plants? Is the *Dionæa muscicapa* to be regarded as malignant, in comparison with ordinary plants?

We therefore feel warranted in inferring that there is no reason to expect any diminution of bloodthirstiness and ferocity from man's adoption of a purely vegetable diet: that he would be morally improved by such a change in any other respect there is not the slightest evidence. Further, even supposing that the rejection of animal food would decrease man's "combativeness," as the phrenologists call it, we are by no means sure that this would be an unmixed benefit.

Here is the place to examine the Vegetarian assertion that drunkenness—or rather the appetite for alcoholic drinks—is merely a consequence of the use of animal food. Now, that a man who is confined to a dry animal diet may feel more thirst than one who feeds mainly upon juicy fruits will not be disputed: but the craving for alcohol is not essentially connected with thirst; it is merely one manifestation of a tendency felt more or less among all people quite irrespective of diet—the love for narcotics. Whether this desire is to be gratified with alcohol, opium, *Cannabis indicus*, betel, or coca, is a mere accident of locality. Now it certainly cannot be contended, with any respect for facts, that the nations which make the nearest approach to vegetarianism are any the less inclined to lap themselves in a temporary elysium, by the use of some drug of this class, than the most carnivorous tribes. Nor is alcohol itself scorned by people whose food consists chiefly of vegetables. The African negro—fed mainly on bananas and maize—quaffs rum as eagerly as the carnivorous red man of North America.

We must now ask what, if any, are the physical benefits to be derived from a purely vegetable diet? Vegetarians contend that by rejecting animal food we should prolong our lives, and increase in strength, health, and capacity for work, whether intellectual or muscular. Such assertions are equally difficult to prove and to refute. If we could divide the people—say of England, or of France, or of Germany—into two sections, each comprising one-half of every class, rank, and occupation, and could feed the one for some few generations on a strictly vegetable regimen, whilst the other were permitted to adhere to its present diet, we might then obtain results capable of deciding the question; but the facts and arguments now brought forward are lamentably defective.

Some few individuals up and down England may have found or fancied themselves the better in health after bidding a last farewell to beef or mutton. We do not dispute the possibility of such cases; but we have not the slightest doubt that quite as many persons could be found who would be benefitted by a more liberal supply of sound animal food. We have known persons who—in the days of '48, when the millennium was to have been suddenly brought in by revolutions and "movements," and articles in the *People's Journal*—gave the vegetarian system a trial. Some of these experimentalists held out for four or five months, others for as many years; but none of them could frankly declare that they experienced any tangible improvement in health, strength, or endurance, and all have since returned to the ordinary diet of their country.

Vegetarians sometimes tell us of particular classes of men in foreign countries who take very little animal food, or none at all, and yet show a wonderful strength and endurance. Such are the porters of Constantinople and the men who—in the mines of Chili—carry up the ore "to grass" upon their backs. That these men are strong and vigorous we do not dispute, but that their strength and vigour are due to their abstinence from flesh, and would be decreased by the adoption of a mixed diet, is nowise proved. These occupations are generally in the hands of something very like a hereditary caste, and we know to how great an extent strength is hereditary. None but hale and hearty men could, at the beginning, enter upon such kinds of work. Those who were at all deficient in strength would either be killed off or would abandon the task, and thus, by a process of "natural selection," a body of men would be formed of remarkable strength, with little reference to their peculiar fare. All that these examples prove, at the utmost, is that a vegetable diet is not under certain circumstances incompatible with health and strength. But what would be the result of taking an invalid, or even an ordinary person in average health, and feeding him—as the Chilian miners are said to be fed—exclusively upon beans and water? We should like to see a few such experiments made, for public instruction, *in corpore vile—i.e.*, upon the wife-beaters, garotters, corner-men, and other the like beings who, worse than useless at present, might thus be made of some service to mankind. But we fear the investigation will never be undertaken in face of the rampant "humanitarians" of modern England.

To return from this digression, we have known vegetarians actually argue in this wise:—The camel is more enduring than the lion or the tiger. Therefore if man would, like the camel, eschew flesh he would become much more enduring than he is at present! There can surely be no need to point out the fallacy of such reasoning. But a counter argument, of equal validity, might be easily framed. Thus we might say that the grizzly bear has greater strength than the bison. Hence if man would adopt a purely animal diet he would greatly improve in strength. The fact is that strength and endurance, in very varying degrees, can be found both among Carnivora and Herbivora. Those creatures which excel most in a short display of strength or speed—such as the lion, tiger, horse, python, &c.—are generally least fitted for protracted exertion.

Let us now proceed to the considerations which indicate that a mixed diet is more beneficial to man in a physical point of view than an exclusively vegetable regimen. No vegetarian nation has ever made any considerable figure in the world. Those, on the contrary, which have been foremost in civilisation, and exercised the greatest influence, have been semi-carnivorous.* Now if animal food were unfavourable to health, vigour, longevity, and rapid increase of population, the very contrary results might be expected. We should see the flesh-eating nations decay spontaneously, or at all events be easily overthrown, whenever they came in collision with vegetarian races. But of such events history gives us no instance. Further, the littoral populations of Western and Northern Europe have always been, to a great extent, fish-eaters; yet they have been eminently hardy and vigorous, and remarkable for longevity. The women of the fishing-villages are often possessed of masculine strength and endurance. Can these facts be easily reconciled with the notion that animal food is injurious or debilitating? In modern England the upper classes—and especially the territorial aristocracy—appear more long-lived than the lower orders; yet they are assuredly not vegetarian in their diet. Again, the trainers of athletes—men certainly not given to forming theories, but guided simply by the results of experience—always insist upon the use of a mainly animal regimen as a preparation for any display of unusual strength or endurance. They have found that fruits, roots, and the like, do not give the support needed by the pedestrian, the race-runner, the oarsman, swimmer, or wrestler. In like manner our medical authorities, when strength is declining, find that it is best kept up by animal matters, such as fresh broiled meat, *extractum carnis*, cod-liver oil, &c.

Taking our stand on such facts we may challenge the vegetarian advocate to point out in what manner, or from what reason, animal food should be less salutary than a vegetable diet. What hurtful matter does it contain? It seems, indeed, to be pre-eminently adapted for the food of the highest-known form of animal life. We see the vegetable world collecting from inorganic matter, and from the decomposing *débris* of former life, certain principles, and elaborating them to a certain extent. The plant then becomes the nourishment of animals, and in their system the processes of selection, concentration, and elaboration are carried still further. The animal then serves for the food of men. We know that not merely the ultimate, but even the proximate, constituents of plants and of animals are very similar. There are vegetable albumenoids closely analogous to those of the animal system. There are vegetable and animal fats scarcely to be distinguished from each other. It is even true that vegetable substances—such as peas, beans, and lentils—may contain a larger percentage of combined nitrogen than the flesh of animals. But as a rule animal matters can be digested and assimilated more easily, with a less expenditure of the resources of the system, than their vegetable analogues. All this points in a direction opposite to the vegetarian theory. But these dietetic reformers cannot openly commit themselves to the view that animal matter is innutritious, indigestible, or injurious, because they tolerate, after all, the use of milk, butter, cheese, and eggs! To declare milk unnatural and injurious as food for a mammalian animal has hitherto seemed to them a somewhat daring statement. But cheese is certainly a less digestible food than beef or mutton, and has even been pronounced one of our national dietetic sins. Yet once admitting these favoured animal substances, the difficulty is to draw a logical boundary. If the egg, why not the chicken? The two are one and the same being, in two different stages of development. The only tangible distinction—viz., that the egg can be eaten without inflicting pain —is, if true, scarcely any ground why its action on the system of the eater should be modified. We are told that milk is a secretion expressly intended for food. Be it so; but to assert that the flesh of the ox is not equally adapted to be the food of man is simply to beg the question at issue. It is even possible to go further. The milk of the cow is undoubtedly the natural food of the calf, but why should it, any more than beef, be pronounced the natural food of man? To rob the calf of its mother's milk is, from a sentimental point of view, perhaps as great an outrage—as decided an infringement of the "normal arrangements of Nature"—as to rob it of its life. To compel a female animal, by artificial treatment, to give milk when she is not suckling her young is likewise "unnatural." It is quite possible that the secretion so obtained may assume a morbid character, and may convey the seeds of consumption, or at least of an impaired vitality, to the human beings by whom it is consumed. Pus cells have frequently been detected in milk. Thus one of the few kinds of animal matter which the vegetarian tolerates is quite as likely to be injurious as those which he denounces and avoids.

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* It is remarkable that our "six-footed rivals," the ants,—the only creatures who at all approach us in civilisation,—are, like ourselves, omnivorous. The animal restricted to one class of food must find itself placed at a disadvantage in the struggle for existence.

Flesh, we are told, is not the "natural food" of man. In reply, we beg for an explanation of the apparently simple phrase "natural food." If we consider every animal species as a something unalterably fixed, destined to inhabit some particular region and to feed upon some especial article, for obtaining and digesting which it has been pre-adapted, the words have a very definite meaning; but if we regard the species as something mutable alike in its organisation, its habitat, and its diet, the natural food of any animal signifies merely the class of substances which we find it for the time being in the habit of consuming. Such substances need not be the best possible, but may often be merely the best procurable. An analogous feature in vegetable life has been pointed out by Dean Herbert. We must not suppose, *e.g.*, that if we find a plant growing on moorlands, that moorland soil is the best adapted to its wants; it might thrive much better in rich, deep, loamy soils—only there it encounters dangerous competitors, which in a thin moorland soil cannot exist at all.

Now if we, bearing these considerations in mind, enquire again what is the "natural food" of man, we shall be forced to admit that he is semi-carnivorous. As a rule he prefers a diet consisting, in part at least, of the flesh of animals. This craving for flesh is not a matter of climate; it is experienced in equatorial regions as decidedly as in semi-arctic Britain, or even in polar lands.* Nay, where animal food is scarce, there cannibalism is most general. It is true that ancient traditions seem to point to a time when man, like the other anthropoids, was purely frugivorous; but we have to do not with pre-historic man, but with man as at present constituted and situated: what he was in earlier stages of his development may be a matter of speculative interest, but can throw no light upon the diet he ought to adopt to-day.

Again, it is urged that we have a natural repugnance to animal food which is only rendered palatable by the artificial processes of cookery: this, also, is an error. The Tartar, when setting out on a journey, will place a piece of raw meat beneath his saddle, and, after a ride of some four or five hours, consume it eagerly, with no other preparation. The German will eat raw ham, *Trichinæ* included, with evident relish. A very large proportion of the meat consumed in England—at eating-houses, hotels, and even in private families—is called "cooked" by an extravagant figure of speech. Raw oysters are never objected to by the daintiest epicure. Nor must it be forgotten, on the other hand, that much of our vegetable food is only rendered fit for use by the arts of the cook. Of the two evils, we should certainly prefer a raw beef-steak to a raw potato, and should find it much more digestible.

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* Bates, Naturalist on the Amazon, vol. ii., p. 214.

We are further reminded that animal food exposes its consumers to a variety of dangers. The ox or the sheep may have been suffering from "rinder-pest," or pleuro-pneumonia; the pork may be trichinised. Flesh of any kind may be passing into decomposition. Sausages may convey the germs of that ill-understood poison which has proved fatal to so many lovers of such questionable dainties. Mussels may, as it seems, capriciously poison one guest and leave ten others unhurt. Poisonous fishes exist in numbers, and we are as yet far from an exact knowledge of the dangerous species. We admit all this to the fullest extent; but we reply that if the path of the flesh-eater is beset with snares, that of the vegetarian is undermined with pitfalls. Putrescent fruits and vegetables are no less common than tainted meat and game, and certainly not less injurious to the eater. Fruits and salads may introduce Entozoa into the system as decidedly as raw ham. Vegetables have their diseases, under whose influence they become decidedly unwholesome. The fungus tribe, like the fishes and Mollusca, comprise both salutary and poisonous species, and exhibit much of the same capriciousness in their action which characterises the latter. Nor must we forget that numbers of persons have perished from mistaking some deadly herb, root, or fruit for an esculent species. Fool's-parsley has been confounded with parsley, the roots of monk's-hood with horse-radish, and the very berries of deadly nightshade have been made into pies! These facts prove not that we should abandon either vegetable or animal substances as a class, but that in the selection of both we should be prudent and wary. To eat raw ham, sausages of unknown origin and history, fishes of doubtful species, and the like, is certainly "tempting Providence." But equally foolhardy is the man who eats fruits "on the turn," fungi gathered by inexperienced persons, or salads and celery from a sewage-irrigation farm.

The teeth and the digestive organs of man, it is said, are indeed in their structure intermediate between those of the Carnivora and of the Herbivora. But this, it seems, proves not that we are adapted for a mixed diet, but that our "natural food" is something holding a middle place between animal and vegetable substances. Such substances, we are further told, are fruits, roots, and seeds—bodies, we must observe, all essentially vegetable, and widely differing in their chemical composition. It is further asserted that as our nearest zoological affinities, the apes, are vegetarians, our flesh-eating habits must be a departure from what Nature enjoins.

In reply to all this we must point out that the organisation of an animal throws a far less clear light on its habits than was supposed in the days of Cuvier. Nor is the matter much more conclusively settled by zoological affinities. Among the bears, for instance, we find the polar bear as purely carnivorous as the lion or tiger. The dreaded grizzly of western North America is scarcely, if at all, less exclusively a flesh-eater. But in tropical and sub-tropical climates we find several bears which live chiefly upon roots, grain, fruits, and honey, and rarely, if ever, consume animal food, except they can find no other. Now between these bears, differing thus widely in their respective diet, we find no well-marked distinction in the structure and length of the intestinal canal, or in the nature of the teeth. Again, among the rodents, we find the hare and the rabbit purely herbivorous, whilst the mouse and the rat are omnivorous—not merely devouring dead animal matter when it falls in their way, but sometimes attacking and killing living animals.* Now the structure of the rat and the mouse certainly gives but very slender, if any, reason to suppose that they are better adapted for an animal diet than the hare or the rabbit. In view of such facts we submit there is nothing either in man's dentition and the structure of his intestinal canal, or in his morphological affinity with the gorilla and the chimpanzee, to prove him unfitted for the use of animal food. He certainly approaches as nearly to the carnivorous type as does the mouse, the rat, or the swine, which latter animal—even in a wild state—is semi-carnivorous. Surely, therefore, all vegetarian arguments drawn from anatomy and zoology may be dismissed as fallacious.

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* That rats will kill and devour guinea-pigs, chickens, cage-birds, &c., is a matter of common notoriety. As regards mice, their predatory habits are less generally known. The present writer, many years ago, caught a field-mouse, and placed it in a collecting-box to carry home as a delicacy for some captive vipers. In the same box were three lizards (*Lacerta crocea*). On arriving at home we found the lizards all dead—each killed by a bite on the throat. It is quite possible that they may, in like manner, overpower their great enemies the viper and the Austrian adder (*Coluber austriacus*), when the latter are benumbed with the winter's cold. Mice, likewise, wage war with scorpions, with varying success.

Perhaps the only remaining argument of the vegetarian party which still requires to be discussed is the assertion that our present system of diet involves cruelty to animals. The question resolves itself into this—Have we the right to take animal life at all? or, to put the matter more plainly, have we the right to exist? The alternative is put before us, to kill or to be killed. We can scarcely move without destroying some minute organism. We are compelled to extirpate, as far as practicable, beasts of prey, venomous reptiles, parasitic vermin, and Entozoa. In our agricultural operations, by digging, ploughing, liming, and draining the soil, we slay unwittingly and incidentally, but not the less surely, legions of worms and larvæ. Not only so, but we are obliged, for the defence of our crops, to wage a constant war against field-mice, rats, hamsters, graminivorous and fruit-eating birds, caterpillars, sawflies, aphides, locusts, earwigs, mole-crickets, weevils, wireworms, slugs, and other animals down to the *Phylloxera* and the *Oidium*. Vegetarianism, therefore, would not exempt us from the necessity of destroying animals to secure, if not to procure, our sustenance. Nay, it is very probable that the numerical amount of life taken directly for our animal diet would sink into utter insignificance compared with the slaughter necessary in the cultivation of our vegetable food. Now, if we are justified in thus killing to defend our crops, does it not seem very hazardous to contend that we may not kill for the direct purpose of eating? True, the enemies of our fields and gardens are generally of a small size and a low organisation. But it would be absurd to permit the death of a locust, and condemn the slaughter of a sheep.

It must further be remarked that those animals which we now rear for food would, if no longer required, be infallibly doomed to extirpation. The hog, the hare, the rabbit, the turkey, duck, goose, &c., could not be permitted to go on increasing in our midst, and consuming the produce of the soil, when no longer able to make us any return in their flesh. The future even of the ox and the sheep would become problematical. It may be very well for Shelley to sing—

> "The dwellers of the earth and air
> Shall crowd around our feet in gladness,
> Seeking their food or refuge there."

But in sober prose the disuse of animal food will mean to many species not emancipation, but destruction. Hence it appears that vegetarianism, so far from putting an end to the havoc which we now make among the lower animals, would leave the greater portion of it untouched, and would rather lessen than increase the amount of animal enjoyment on the earth.

Summing up the total result of our enquiries, we find that the abolition of our fisheries and the disuse of our pasture-lands must render the promised economical advantages of vegetarianism highly problematical. We see no good reason to expect either moral or physical benefit, to any marked degree, from the dietetic reform. We see that the strictest vegetarianism would not exempt us from the wholesale destruction of animal life. Nor can we find any soundness in the lines of argument hitherto employed to show that the flesh of animals is not a fit food for man.

Until some totally different evidence shall have been adduced we cannot consider our present mixed diet one of the causes of the woes that afflict humanity.

Though unable to accept the theories of vegetarians, it must not be supposed that we entertain any hostility to their practice. If any man finds that a purely vegetable diet is better suited to his health, his sentiments, or even to his pocket than a mixed regimen, we do not for a moment contest his right to act upon the results of his experience. Nor can we demur to any temperate and rational attempts at bringing over others to the same way of thinking; but unfortunately every social "movement" takes the earliest opportunity to constitute itself an intolerance, and to become offensive in every sense of the word. To this rule vegetarianism forms no exception; its advocates are prone to question the sincerity of all who reject their arguments, and to accuse them of wilfully and deliberately shutting their eyes to the truth. Certain ugly names, such as "blood-eater,"* are also hurled at the heads of persons who still feel themselves justified in adhering to the roast beef of Old England. Should the party gain sufficient strength, we should not feel at all surprised if the attempt were made to suppress flesh-eating by the power of the law. If we might presume to advise, we should suggest to vegetarians the propriety of being a little less selfish, and not seeking to force their good things upon a reluctant public. Let them remember that virtue itself is not exempt from the laws of supply and demand. If the vegetarian system possesses half the merits claimed by its advocates, those who carry it out in practice will soon evince such an unmistakable superiority to their neighbours that imputations, abusive epithets, and all the stock in trade of modern professional philanthropy will be utterly needless.

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* A flesh-eater is not necessarily a blood-eater. The unwholesomeness of blood does not detract from the wholesomeness of meat any more than the dangerous nature of the sap of the cassava root proves its insoluble portion to be improper for food. It is too often forgotten that the blood, though conveying nutrition to all parts of the body, is also charged with the *effete* matter thrown off by the tissues on its way to the organs of excretion. It is, therefore, impossible to take blood from an animal without obtaining a mixture of sound matter with that which is utterly unfit for food. It is a humiliating fact that in a certain English seaport the blood of the cattle there slaughtered, instead of being utilised by mordant-makers and manufacturers of chemical manures, is actually consumed as food by the unfortunate inmates of the workhouse!

III. RECENT CHEMICAL RESEARCHES.

By M. M. Pattison Muir, F.C.S.

IN the following paper we propose to give an account of some of the most important recent researches bearing upon points of theoretical interest in Chemical Science.

I. *Dissociation of Elements.*

From the time of Prout to the present day the theory that the so-called elementary bodies are really compound substances has found supporters. Mr. J. Norman Lockyer has lately put forward an hypothesis, concerning the dissociation of elementary atoms by the intense heat of the sun and certain stars, which well merits the attention of chemists.

Inasmuch as all solids give continuous spectra, while all vapours produced by the high-tension spark give line-spectra,—inasmuch, further, as compounds give continuous spectra, but are resolved by the high-tension spark into their constituent elements,—the conclusion may be drawn that an element in a solid state is more complex than the same element in a state of vapour.

The following five stages in spectrum complexity are distinguished:—

First stage of complexity ..	Line-spectrum.
Second ,, ,, ..	Channelled space-spectrum.
Third ,, ,, ..	Continuous absorption at the blue end.
Fourth ,, ,, ..	Continuous absorption at the red end.
Fifth ,, ,, ..	Unique continuous absorption.

These different spectra are believed to be due to different molecular aggregations. The spectra of the non-metallic elements do not exhibit the same order of change, with increase of temperature, as those of the metallic elements. Sodium exhibits a line spectrum at a dull red-heat; at the same temperature the spectrum of sulphur is characterised by a continual absorption in the blue: as the temperature increases the line spectrum of sodium remains unchanged, but the spectrum of sulphur gives place to a channelled space spectrum, which remains until the temperature of the electric arc is reached, when a line spectrum makes its appearance. There are therefore three stages of non-metallic spectra corresponding to three stages of heat, but these three stages do not correspond with the stages of the spectra of metallic elements: hence the inner structure of the non-metals probably differs from the inner structure of the metals.

The spectra of the substances which are present in the reversing layer of the sun are all line spectra; hence it is inferred that these substances are all in a similar state of aggregation. But we saw that at comparatively low temperatures non-metals are characterised by channelled space spectra, which are believed to indicate a more complex inner structure than is shadowed forth by line spectra. The inference to be drawn from these facts (admitting the connection between simple spectra and simple molecular constitution) seems to be that the formation of those molecular aggregations which are presented to us by the non-metallic elementary bodies on the earth is not possible at the temperature of the sun.

This hypothesis is put forward by Mr. Lockyer merely as a means for guiding future work: from the chemical view point it is exceedingly interesting. That the non-metallic molecules should be more ready to undergo change than the metallic molecules is in keeping with many known chemical facts. The phenomenon of allotropy occurs to a considerable extent among non-metals, and it is almost certain that allotropic changes are always accompanied by variations in molecular aggregation. The change of ordinary into amorphous phosphorus, and *vice versa*, would seem to depend essentially upon the tension of the phosphorus vapour; and what does this statement imply but that, under certain conditions, the molecules of phosphorus are so packed together as to exhibit to our senses the phenomena associated with what we call ordinary phosphorus, while under other circumstances the state of molecular aggregation is such as that the body exhibits the phenomena associated with amorphous phosphorus?

Mr. Lockyer defines a metal, provisionally, as a substance whose absorption and radiation spectra are the same, and non-metals as substances in which these two spectra differ.

Furthermore, these researches introduce a new definition of the "atom" as being that mass of matter which gives us a line spectrum, while the mass of matter (non-metallic) which gives us a channelled space spectrum is called a "sub-atom."

The plasticity of non-metallic molecules is a subject to which we shall have occasion to make further reference.

II. *Atomic Weights of the Elements.*

Of the various methods which may be employed for ascertaining the atomic weight of any element, that which is based upon the determination of the density of the element when in a state of vapour and the densities of the greatest possible number of gaseous compounds of that element holds the first place. By checking the results of such vapour density determinations by means of gravimetric analyses we are able to fix on the maximum atomic weight of the element. As a means of determining the atomic weight of those elements which do not assume the gaseous condition at any attainable temperature, the method of specific heat has generally been adopted. This method is based upon the fact that the average product obtained by multiplying atomic weight into specific heat (called atomic heat) is, in the case of the solid elements, represented by the number 6·3. The fact that the atomic heats of the three solid elements, silicon, boron, and carbon, are represented by the numbers 4·8, 2·7, and 1·8 respectively, has rendered the application of the method of specific heats to atomic weight determinations somewhat unreliable. If these three elements deviate so widely from the general rule, what guarantee have we for believing that a similar deviation does not occur in the case of other elements the determination of the atomic weight of which is as yet an unsolved problem?

Prof. Weber, of Hohenheim, has made a number of exceedingly accurate determinations of the specific heats of carbon, boron, and silicon, from which he arrives at the result that these elements obey the law of Dulong and Petit. On comparing the numbers given by different observers for the specific heats of the three bodies in question, it was ascertained that these numbers represented the specific heats for different intervals of temperature, and that the greater the interval of temperature for which the determination was made the greater was the number representing the specific heat.

In a preliminary series of experiments Prof. Weber showed that the specific heat of diamond increases, with increase of temperature, more quickly than that of any other substance—the values at 0°, 100°, and 200°, being almost in the ratios 1 : 2 : 3. Hence it follows that there is a close relation between the specific heat of diamond and the temperature, and that these quantities increase simultaneously. The value of this increase, however, diminishes at a red-heat and upwards to a white-heat, until it becomes but a seventeenth part of what it was for the temperature-interval 0° to 100°.

From a red-heat upwards the value of this increase is not greater than that of the increase noticed in the case of the elements which obey the law of Dulong and Petit.

The actual numbers representing the specific heat of diamond at high temperatures are as follows:—

Temp.	Spec. Heat.
606° C.	0·4408
806°	0·4489
985°	0·4589

By multiplying these numbers by the generally received atomic weight of carbon we obtain a product varying from 5·2 to 5·5. Other elements which have small atomic weights give very similar numbers: thus the product obtained by multiplying the atomic weight of aluminium into its specific heat is 5·7; of phosphorus, 5·5; of sulphur, 5·5; &c. Crystallised boron shows a like increase in specific heat with increasing temperature: at —39·6° the number expressing the specific heat is 0·1915, while at 233·2° it is 0·3663. By reasoning from these numbers the conclusion is drawn that at a red-heat the specific heat of boron attains a constant value equal to about 0·5. If this number be multiplied by 11, the generally-received atomic weight of boron, we obtain the product 5·5, which expresses the atomic heat of this element.

Similar researches, carried out with crystallised silicon, have led to similar results, viz., that the specific heat of silicon is a function of the temperature, and that at about 200° it attains a constant value: 0·2029 is the experimental number representing the specific heat of this element at the temperature 232·4°; 28, which is the received atomic weight of silicon, multiplied into 0·2029 gives us the product 5·68.

The three elements, carbon, boron, and silicon, are therefore no longer to be considered exceptions to the generalisation of Dulong and Petit; and we may, with more certainty than heretofore, apply this generalisation in the determination of disputed atomic weights. A modification must, however, be made in the statement of the law, thus:—"The specific heats of the solid elements vary with the temperature: for every element, however, there is a point (T_0) from which the variation in the specific heat, with increasing temperature, is entirely insignificant. The product of the atomic weight into the value of the specific heat (estimated at temperatures so that $T > T_0$) is, for all the elements, a nearly constant number varying from 5·5 to 6·5."

The bearing of Prof. Weber's researches upon the specific heat of allotropic modifications of the same element, and also on the question of varying chemical value of the same element in its compounds, will be discussed hereafter.

Changes have been proposed by Mendelejeff in the numbers which express the atomic weights of yttrium and erbium, of cerium, lanthanum, and didymium: these proposed changes are based upon considerations of the relations which appear to exist between the atomic weights and the properties of the elements. These relations exhibit the form of a periodic function. If the elements be arranged in order of their atomic weights, we may divide them into groups the properties of the members of each of which exhibit a regular variation as the atomic weights increase. Now, if the atomic weights generally accepted for yttrium, erbium, cerium, lanthanum, and didymium be the true atomic weights of these elements, we find that these bodies do not occupy the positions in the groups which, judging from their properties, we should expect they would do. But as we are certain, so far as investigation has gone, of many of the properties of those elements, while we are not at all certain of their atomic weights, Mendelejeff proposes to alter the latter, so that the elements may fit into those places which, according to his theory, they ought to occupy.

In order that the atomic weights of these elements should be more definitely settled it is necessary that the specific heats be determined. Mendelejeff states that he has determined the specific heat of cerium to be 0·05: this number multiplied into 138 (the number proposed for the atomic weight of this body) gives 6·9, which is in keeping with the average atomic heat of the elements. Nilsson is at present engaged in the preparation of considerable quantities of these metals, with the view of determining their specific heats.

III. *Classification of the Elements.*

We have already referred to Mendelejeff's system of classification according to atomic weights: the more commonly employed systems of classification are based upon the chemical value or valency of the individual elements. The researches of Michaelis, of Meyer, and of others, have important bearings upon the general question of valency.

That the valency of each chemical element is invariable throughout the whole of its compounds has been and is maintained by many of our most distinguished chemists: the doctrine opposed to this—viz., that the valency is a varying quantity—has also been upheld by able supporters. Before we are in a position to discuss this question it would be well that we should have a clear idea of what chemical valency means. Adopting the usually-received definition of "atom" and "molecule," we say that one atom of carbon is capable of combining with four atoms of hydrogen, while one atom of oxygen is capable of combining with only two atoms of hydrogen. The carbon atom is therefore said to be "equivalent to" four hydrogen atoms. But we must remember, as Mills has pointed out, that carbon and hydrogen atoms have never been compared as to the work they can do under certain circumstances, and that the relations of their potential energies have yet to be determined. We are very apt to confuse two things which are, most probably, totally distinct—affinity and chemical valency.

Michaelis imagines a carbon atom performing vibrations so that there are four positions during each complete vibration in which it is possible for the atom to come within the sphere of action of another atom: he imagines an oxygen atom performing vibrations so that there are but two positions during each complete vibration in which it may come within the sphere of action of another atom. Or we may imagine the carbon atom as exercising force in four different directions, while the oxygen atom exercises force in but two directions. The number of positions of advantage, as we might call them, or of lines of force, may then be independent of the total force exercised. We may imagine an atom exercising force in but two directions, and nevertheless exercising an absolutely greater *amount* of force than another atom the directions of the exercise of whose force amount to four in number. The total force exercised is therefore the affinity, and is dependent upon the nature, position, &c., of all the atoms in the compound: the number of directions, or the number of positions, in which this force may be exhibited is the chemical valency of the atom.

Now it is not necessary to suppose that the total force is exercised equally in each of the various directions; it is possible that a greater amount may be exercised in direction 1 than in direction 2: hence there may be a difference in the nature of a compound according to the direction in space of the lines joining the centres of the component atoms.

The mutual action of atoms is supposed by Michaelis to be for certain distances an attractive action, but for very small distances a repulsive action. Hence when two atoms come within the sphere of one another's action they approach one another until the repulsive action comes into play, when they are repelled until the repulsion gives place to attraction, and so on. But under certain circumstances, which may easily be imagined, the atoms are driven so far asunder that return within the sphere of one another's action is no longer possible; the compound, therefore, is broken up.

In this view chemical valency is constant for each element, inasmuch as the number of directions in which the chemical force may be exerted is a constant number; but the force need not be equal in amount in each direction, nor need it be always expended in doing work in each direction when the element unites with others to form a compound substance. Furthermore, the valency is independent of the total affinity of the element, so that when we speak of tetravalent carbon and divalent oxygen we do not mean it to be inferred that the potential energy of the carbon atom is double that of the oxygen atom, but merely that the carbon atom is able to exercise its total energy in twice as many directions as the oxygen atom is able to do.

The chemical valency of the elements may therefore form the basis of a system for their classification. In order to determine the valency of any element it is necessary to take into account the whole of its compounds so far as we are acquainted with them. But inasmuch as we cannot determine with certainty the molecular weights of any compounds other than those which are gaseous at attainable temperatures, we must limit our attention to such compounds. Conclusions concerning valency, drawn from a consideration of solid or liquid compounds, are very misleading.

The existence of so-called molecular compounds is liable to mislead in the determination of the valency of an element. No definition, which may be easily and exactly applied, of a molecular compound as distinguished from an atomic compound, has yet been given. Michaelis proposes to adopt the rule that when a doubtful compound yields an atomic compound by double decomposition it is itself an atomic compound. Inasmuch as ammonium chloride yields ammonia (the molecule of which is represented by the formula NH_3) by the action upon it of lime, we should probably be obliged to class this compound among those which are atomic. But we know that the action of heat upon ammonium chloride resolves it into ammonia and hydrochloric acid; is it not, then, very probable that the seeming double decomposition which takes place between sal-ammoniac and lime is in reality an action between hydrochloric acid and lime, and that the first action of the heat is to decompose the ammonium chloride with the production of ammonia, which escapes, and hydrochloric acid which acts upon the lime? In the case of such compounds as ammonium chloride it is very difficult to determine whether cases of seeming double decomposition really belong to this category or not.

But if we agree with Michaelis's general view of valency —viz., that it is measured by the number of directions in which the energy of a chemical element is exercised, but that the amount of energy need not be the same in each direction—we may imagine that the weaker lines of force are those which are concerned in the formation of the so-called molecular compounds. Let us take the case of sal-ammoniac. Ammonia gas and hydrochloric acid gas, when brought together in proper proportions, are no longer to be distinguished as such; their properties become merged in those of the new solid body formed. But if this new body be heated it is resolved into ammonia and hydrochloric acid; if the mixture of heated gases be allowed to cool the solid body is re-formed. Let us suppose that there are five positions in which the nitrogen atom can exercise its chemical energy during each complete vibration. In ammonia the nitrogen atom is brought within the sphere of action of three hydrogen atoms, and we may suppose that during each complete vibration there are three positions at which the attractive force of each hydrogen atom in turn changes to a repulsive force; the nitrogen atom therefore oscillates about these three positions, and at the same time about two other positions at which it is possible for other atoms to exercise an action upon it. And it is at these two latter positions that the action of the hydrochloric acid molecule is exhibited. But inasmuch as we know that the total energy of chlorine is large, although it be exerted in but one direction, while the total energy of nitrogen is comparatively small, it seems difficult to imagine that the hydrochloric acid molecule will be split up into its constituent atoms, and that these will then each oscillate about the nitrogen atom. But is it not possible to avoid this difficulty by supposing that there is such a thing as molecular as well as atomic valency?—that although in the hydrochloric acid molecule the lines of force of its atom are mutually satisfied, there may be directions in which the molecule, as such, may exert a certain amount of chemical energy? On this view we may picture to ourselves the hydrochloric acid molecule oscillating about these two points, at which the nitrogen atom is still free to exercise chemical force. But the force exercised at these points is, by supposition, less than that exercised at the other three points; therefore when the compound is heated the hydrochloric acid molecule is easily driven beyond the sphere of action of the nitrogen atom, and we have the phenomenon of dissociation.

Whatever theory may be adopted of actions such as this, it is evident that, in the determination of the valency of an element, attention should be paid to the whole of its compounds, but that reliable conclusions are only to be drawn from those which can exist in the gaseous form.

Researches bearing upon the valency of the individual elements have been carried out of late by various observers. Many disputes have arisen as to the valency of the elements which constitute the nitrogen group, so that an especial amount of interest attaches itself to any new researches into the constitution of the compounds of the elements. Michaelis concludes, from general observation of a large number of its compounds, that phosphorus is a pentavalent element. Wurtz has determined anew the vapour density of phosphorus pentachloride, by diffusing the vapour of this compound into a space filled with the vapour of phosphorus trichloride. By this means the dissociation of the pentachloride was prevented. The number 7·226 represents the average vapour density calculated from the results of twelve experiments; the theoretical vapour density for PCl_5 (two volumes) is 7·217. We have in this determination an almost certain proof of the pentavalency of phosphorus. Another proof that phosphorus is really a pentavalent element has been supplied by Thorpe, who has succeeded in preparing a gaseous pentafluoride of phosphorus, PF_5, by the action of arsenic trifluoride upon phosphorus pentachloride: the new compound acts upon glass, and is soluble in water, but may be collected over mercury. It seems, then, very probable that the elements of the nitrogen group are really pentavalent.

Exceedingly interesting results are certain to follow from researches into the total affinity force exercised by the elements as distinguished from their chemical valency. Hitherto this field of enquiry has had but few cultivators, probably on account of the difficulties which beset the problems presented. A step in advance has been made by Wright, who has calculated the affinity of the elements, in several compounds, in terms of the heat gained or lost during the formation of the compound from its constituent elements; but as Wright's determinations have a more immediate bearing upon the questions to be considered when speaking of compound bodies, we shall defer a more detailed account of them to a later part of this paper.

It is well known that several of the elements can exist in various so-called allotropic forms: these forms seem to be connected with the original form of the element in somewhat the same way as isomeric compounds are connected with the primary forms from which they are derived.

We have already seen how Lockyer's spectrum researches lead to the idea that the molecules of the non-metallic elements are possessed of a large amount of plasticity: the facts noticed in the study of the chemical history of these molecules fully confirm this theoretical deduction. If oxygen be subjected to the action of the silent discharge it exhibits many new properties, yet we know that from the substance thus formed, oxygen—and oxygen only—can be obtained. We are obliged, therefore, to imagine that a change has taken place in some way in the inner structure of the oxygen molecule, or that under the altered circumstances that molecule is endowed with different potential energy from that which it formerly possessed. The formation of one or other of these molecules is conditioned by the temperature; at a temperature of 300° C. ozone is decomposed with the formation of common oxygen. As the general action of heat is to cause greater freedom of molecular motion, we may perhaps conclude that the atoms in the molecule of oxygen are possessed of a greater freedom of motion than those in the molecule of ozone. So, also, we know that if ordinary phosphorus be heated to a certain point, in an atmosphere incapable of acting upon it chemically, amorphous phosphorus is produced; but if the temperature be increased the production of amorphous phosphorus ceases, and that which is formed is re-transformed into the ordinary variety. This action is clearly conditioned by the temperature, and therefore by the tension of the phosphorus vapour. It is found that for a given temperature there is a certain fixed tension of the vapour required to hinder the transformation of ordinary into amorphous phosphorus. As the temperature increases, however, the transformation is again set going, until the tension of the vapour increases to such an extent as again to stop the action. If phosphorus be heated in a confined space, different parts of which are maintained at different temperatures, the tension of the vapour will prevent the formation of the amorphous variety, except at the hottest parts of the space. May we imagine that some such state of affairs prevails in the atmosphere of the sun and of the hot stars?—that the atomic alteration, shadowed forth by Mr. Lockyer's researches, which appears to take place in non-metallic molecules, consists not in the decomposition of these molecules with the production of simpler forms of matter, but in the production of isomeric or allotropic forms of them with which we, on the Earth, are as yet unacquainted? —that at ordinary temperatures the tension of the vapours of the elements is sufficient to stop this action, but that at the high temperature of the sun's atmosphere the transformation takes place?—and that, as these allotropic forms ascend to cooler parts of the sun's atmosphere, the tension of their vapour is able to bring about the re-formation of those molecules which are known to us on the earth?

That quantities of energy are absorbed or evolved in the passage of one allotropic form of an element to another is evident from the results of experiments made by Mitscherlich and others upon the forms of sulphur. The conversion of 1 grm. of dissolved octahedral sulphur into insoluble amorphous sulphur is attended with the evolution of 12,800 heat-units. A careful study of the loss or gain of energy, in the reciprocal formation of allotropic varieties of the non-metallic elements, would doubtless lead to most important results.

Weber has found that the specific heats of the various allotropic forms of carbon differ at low temperatures, but that at those temperatures at which the optical differences begin to disappear the differences in specific heats disappear also. At high temperatures carbon has but one specific heat; hence it exists apparently in but one form.

The facts about allotropic modifications of elementary bodies may possibly have an influence upon the constitution of compounds. Weber concludes from researches, a full account of which has not yet been published, that the specific heat of carbon in combination varies according to the nature of the non-carbonated portion of the compound molecule: he professes himself unable to explain such variations, except on the assumption that the carbon atom is possessed of great plasticity, and that it exists in combination in different allotropic forms. Gautier has also obtained compounds of phosphorus in which that element appears to exist in the amorphous condition; while Houzeau and Renard, by the action of ozone upon benzene, have produced a peculiar body which appears to contain oxygen in its allotropic form of ozone. We may therefore suppose that in the formation of isomeric compounds a transformation of one allotropic form of the dominant atom into another takes place, and that—inasmuch as such a transformation is accompanied with changes in the potential and kinetic energy of the atom—the compounds will differ from one another in the amounts of energy which they contain, although the valency of the individual atoms may be the same in all.

But considerations such as these lead us to speak of—

IV. *Groups of Compounds.*

And here we are, of course, met by the great fact of the existence of isomeric compounds. We must regard the molecules of all substances as material systems. Now Prof. Clerk-Maxwell has said that when we can explain any phenomena as changes in the configuration and motion of a material system, we have given a complete dynamical explanation of these phenomena. We must therefore endeavour to explain the phenomena of isomerism as changes in the configuration or motion of the molecules of the isomeric bodies. The ordinary way of representing such changes is by placing the symbols which represent the atoms of the molecule in different relative positions in space in the different formulæ. Isomerism in this view is caused by a change in the position of the atoms. But we must remember that no formula can give us a true insight into the constitution of the molecule; the atoms are in a state of motion, each performing its own definite oscillation: our formulæ represent them as at rest. Another explanation of isomerism has been sought for by supposing that different amounts of energy are concerned in the production of isomeric bodies, and that these bodies therefore differ in the potential energy which they contain. Now if a greater amount of energy has been expended in the formation of the body A than in the formation of its isomer B, what has that energy been employed in doing? Surely in altering the relative motions and configurations of the atoms. Therefore both views are perhaps most fittingly presented to us in the formulæ which are in common use. If the motions and configurations of the atoms have been altered (as our ordinary formulæ say they have) energy must have been expended in bringing about this alteration, and therefore these formulæ indirectly express this fact. Various physical considerations tell us that there is a difference in the inner constitution of isomeric bodies; their boiling-points are different, and Thorpe has shown—in a preliminary paper read to the British Association at Belfast—that their specific volumes also vary.

The propriety of attempting to express the constitution of such a body as a molecule upon a flat surface has been questioned by Van't Hoff; but it is evident, as Lodge has shown, that "any configuration in space may be projected on a plane surface, and the only difference between such a projection and a diagram drawn directly on the plane will be—that in the latter the bands are not usually made to cross each other, whereas in the former they will be very liable to do so."

A consideration of groups of compounds leads us to speak of recent attempts to systematise inorganic chemistry.

Lothar Meyer—adopting Mendelejeff's view, that the properties of each element are expressed as a periodic function of its atomic weight when the elements are arranged in the order of their atomic weights—has endeavoured to deduce general formulæ which shall express the composition of series of salts, thus introducing the homologous and heterologous series of organic into inorganic chemistry. He has expressed the composition of a number of hydrates and other salts by the general formula $H_{2n}X_2O_{v+n}$, where X represents the atomic weight of an element, v its valency as deduced from the highest oxide, and n a whole number, which generally does not exceed 4. The following examples will illustrate the application of the formula:—

	$v=1.$	$v=2.$	$v=3.$
$n=0$	Na_2O	Mg_2O_2	Al_2O_3
	Cu_2O	Zn_2O_2	B_2O_3
$n=1$	$H_2Na_2O_2$		$H_2Al_2O_4$
	$H_2K_2O_2$		$Mg_2Al_2O_4$
	$H_2Cr_2O_2$		&c.
	&c.		
$n=2$		$H_4Mg_2O_4$	$H_2Ag_2B_2O_5$
		$H_4Ca_2O_4$	
		$H_4Ba_2O_4$	
		&c.	

The formulæ in this table do not always agree with those in general use; but it must be remembered that we are altogether in ignorance of the true molecular weights of the greater number of these compounds. Similar formulæ may be deduced to express the composition of other salts; but, as Meyer points out, we need many new researches into the too much neglected compounds of inorganic chemistry before we shall be able to introduce a complete system of classification of compounds.

With regard to the notation of compounds, we have already seen that the formulæ in general use imply that a certain amount of energy has been expended in the formation of one compound from another. Wright has studied somewhat in detail the relations which exist between affinity and structural formulæ: he has sought to measure affinity by the number of heat-units gained during the coming together of several forms of matter, so as to form a given weight of a compound body. Many interesting generalisations have been deduced, important among which are the following:—

The production of ethers and steam from alcohols is accompanied by heat absorption.

The oxidation of alcohols to acids and steam is accompanied by heat evolution.

In the substitution of the group CH_3 for the hydrogen belonging to the hydrocarbonous part of a radicle heat is evolved, while in the substitution of the same group for the hydrogen belonging to the hydroxylic (OH) part of a radicle heat is absorbed.

The replacement of H_2 by O is accompanied by heat evolution.

These results open the way to most interesting considerations regarding the work done in the formation and transformation of chemical compounds; but before any generalisations can be safely made a large amount of experimental data must be accumulated.

Among the general results which have been deduced by Wright we find the following statement:—"If an operation be performed such that heat is evolved during its performance, the resulting product boils at a higher temperature than the original substance, and *vice versa*." This statement is in accordance with the views of Mohr, and also with those promulgated some years ago by Odling. If we understand by the evolution of heat the loss of molecular motion, it follows that the resultant must be more viscous, so to speak, than the substance producing it, and must therefore boil at a higher temperature. The subject of the influence of heat upon chemical decomposition has been studied by Mohr. In order that a chemical compound may be dissociated by heat alone, it is necessary that one or more of its constituent elements shall be gaseous at attainable temperatures. When the gaseous element or elements became combined, so as to produce a non-gaseous compound, a quantity of heat was evolved,—that is, molecular motion was arrested: the process of dissociation will therefore be accomplished if the quantity of heat lost on combination be restored, because the molecular motion of the constituents of the compound will then be so great that they will no longer hold together. If a large quantity of heat be evolved in the formation of any compound we find that it is hard, or in some instances impossible, to decompose that compound by heat alone. In the formation of carbon disulphide from carbon and sulphur heat is absorbed, because the elements are transformed from solids into liquids: now carbon disulphide may be decomposed by the heat of an ignited platinum spiral, but in this experiment heat is evolved, the amount being equal to that formerly absorbed in the liquefaction of the carbon and of the sulphur.

Looking at the recent researches in chemistry as a whole they all point to the advance of the time when chemistry shall have become a mathematical science, whose laws shall be formulated with the greatest exactness, and whose results shall allow of the application to them of mathematical reasoning.

IV. SIDEREAL ASTRONOMY: DOUBLE STARS—FAR-OFF WORLDS.

By Camille Flammarion.

I. *General Considerations.*

IN the depths of the heavens, amongst the varied stars which shed their silent light above the regions of the starlit night, the eye of the telescopic investigator discovers stars of a particular character, which differ from the ordinary stars in their appearance, as also in the part they play in the universe. Instead of being simple, like the greater majority of the stars of heaven, they are double, triple, quadruple, multiple. Instead of being white, they often sparkle with coloured light, offering in their strange couples admirable associations of contrast, in which the astonished eye sees the fire of the emerald wedded with that of the ruby, that of the topaz with that of the sapphire, the diamond with the turquoise, or the opal with the amethyst, glittering thus with all the shades of the rainbow. Sometimes the wonderful stars which form these celestial couples repose in the vault of infinity, fixed and unchangeable; and for more than a century that astronomers have been anxiously contemplating and watching them, they have not varied their relative position with regard to each other: as the searching look of the patient William Herschel surprised them there a hundred years ago, so we find them to-day. Sometimes, on the contrary, the two associated stars gravitate round their common centre and turn round each other, the weakest round the strongest, rocking on the wing of attraction, as the moon moves round the earth and the earth round the sun. A certain number of these couples have already made many complete revolutions under the eyes of observers, the duration of these revolutions differing with each couple, and offering the greatest variety of periods, from a few years only up to thousands of years, and even to hundreds of centuries. Our small terrestrial calendar does not extend its empire to these far-off worlds; our ephemeral periods, our ant-like measurements, are strangers to these grandeurs; the earth is no longer the measure of creation, and our most sacred eras are unknown in the heavens. The study of these stellar systems constitutes one of the vastest and most interesting problems of the contemporary astronomer; it has, however, remained stationary, and, notwithstanding the considerable number of accumulated observations, no one astronomer has yet tried to gather up the harmony of all these systems, to search for differences which can exist between them, and (except some partial attempts) to catalogue on the one hand the double and multiple stars which have a certain orbital movement, and on the other hand those groups whose movement is not orbital, but rectilinear, and caused by perspective, owing to the displacement of one of the stars situated by accident before another farther off and immovable.

Each star being a gigantic sun, shining with its own light, a focus of attraction, of heat, of activity, and life, the problem presented to the human mind by these systems of multiple suns is, without doubt, one of those which can most absorb the imagination, fire the thoughts, and affect even the heart of a philosopher. What part does gravitation play in these solar systems, so different from ours? What is the numerical importance of these systems in the sidereal world? What is their mode of distribution in the universe? What links have they to simple suns like ours? What is the nature of their light, often so strange and so fantastic? To what respective distances can the stars be associated and governed in common by a proper movement in space? What is the condition of the planetary systems which can gravitate round these double suns? What can be the physiology of these planets, governed, illuminated, heated, alternately and simultaneously, by two suns of different masses, of different distances, and different lights? And, finally, what are the wonderful and extraordinary conditions which can be brought to life on these unknown worlds, lost in the depths of the fathomless heaven? Those are the questions which have occupied my thoughts, and which I have tried to elucidate successively; such is the subject of a great work which I have ventured to attempt, and of which the extent has been much more considerable than I had at first supposed. I have been able, happily, to have in my hands nearly all the observations made on double stars since the commencement; there have been more than a hundred and fifty thousand observations, both of angles of position and of angular distances,—that is to say, more than three hundred thousand together. I have compared them all, and by these comparisons I hope to be able to determine the different species of double stars. The total number of multiple stars of all kinds discovered to this day would reach to 10,487. Of these, a very remarkable number already have been proved to form true physical systems, possessing certain orbital movement. Others remain fixed in the same position. Others are carried away into space by a common movement, and traverse immensity with a bewildering velocity. Others sparkle with a changing brightness, and we see their mysterious light sometimes diminishing and sometimes augmenting. Others even are completely extinguished. My earnest endeavour will be to sum up here in one rapid study, and in a form accessible to all clear minds, the whole of the conclusions to which these researches have conducted me; to explain these phenomena in a picture which will not be too unworthy of these wonders; and to exhibit before the eye all the power and the splendour of these far distant suns, which govern in the depths of space unknown worlds and existences.

II. *History of Double Stars.*

Double stars as such were not known before last century. However, one finds in ancient astronomical works many mentions of stars very near together, which they termed double. Ptolemy himself, in the "Almageste," describes one in τοξοτης (Sagittarius*), which he named διπλους, double. One sees it, in fact, double to the naked eye, and it is composed of two stars very near together (ν^1 and ν^2). They are of the fifth magnitude, and separated by 14 minutes of arc. The star Mizar, or ζ of the Great Bear is alike described from the greatest antiquity as double, or rather as being accompanied by a little star, visible also to the naked eye to those with good sight. The Arabs call it Saidah,—that is to say, *the test*, because they used it to test their sight. It is also named Alcor, and its companion is of the fifth magnitude, and is situated at 11 minutes of arc from Mizar, whose brightness, which is of the second magnitude, eclipses the smaller from ordinary sight. The other stars seem equally joined two and two, and appear to the naked eye as apparently double. Such, also, are α^1 and α^2 of Capricornus, which are—the former of the first magnitude, the latter of the third, and distant from each other 6′ 13″; good sights (that of the German astronomer Heis, among others) can separate them: ν^1 and ν^2 of the Crown, of fifth magnitudes, separated by 6′ 19″, a little more difficult to divide into two with the naked eye; θ^1 and θ^2 of Taurus in the Hyades (5′ 37″); π^1 and π^2 of Pegasus; δ^1 and δ^2 of the Lyre; ϵ^1 and ϵ^2 of the same constellation. Another couple could be added to the preceding; it is α^1 and α^2 of the Balance, whose separation is of 3′ 49″; but the principal star being of the third magnitude, and the second of the sixth, I doubt whether it can be divided with the naked eye, and I know nobody who can do so. Do these apparent couples constitute themselves true binary systems? This is not probable. In general double stars are incomparably closer. However, it must not be affirmed that their connection is only accidental, and due only to chance and to perspective. We shall find farther on examples of true common movements: we are justified in thinking that these stars may be associated together, although they may be more removed still from others on the celestial sphere.

The study of these double stars only dates from telescopic investigations. By a curious chance the first star separated by the telescope is that which was most remarkable to the naked eye, as being apparently double,—Mizar,—which is not only accompanied by Alcor, but is itself double. It is composed of two stars, separated by 14 seconds of arc from each other; the brightest is of the second magnitude, and its companion is of the third. This star was recognised as double for the first time by Riccioli, in the middle of the seventeenth century. In 1700 Gottfried Kirch and his learned wife Maria Margareta described it, and again drew the attention of astronomers to it. Bradley, Mayer, and Herschel observed it afterwards. In comparing the observations made during two centuries, we find that the two stars have hardly changed position with regard to each other. The angle increases slowly. William Herschel had concluded that it was diminishing, but this is an error of the illustrious astronomer. It is possible that the three stars (the double and Alcor) form a triple system. They are all three animated by one common movement in space.

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* In a Latin translation which I possess of the "Almageste" (Venice, 1828) this star is thus described:—"Quæ in oculo est nebulosa et bina."

The second star separated with the telescope is Mezartim, γ of the Whale, which was separated by Hook when observing the comet of 1664. The principal star is of the 4½ magnitude, the other of the fifth; their distance apart is 9 seconds. Their relative position has not varied since the earliest observations.

The third star separated is that which has since become one of the most interesting, on account of its parallax—our neighbour α Centauri. It was described in 1709 by Feuillée, at Lima. Its two components turn rapidly round each other, following a much elongated ellipse, half the principal axis of which measures 14″. Since the time of its discovery there have been more than two revolutions accomplished. We give farther on the elements of this magnificent orbital system, of which the distance from the earth is eight trillions three hundred and seventy-six milliards of leagues.

In proportion as the attention of astronomers has developed and has carried them nearer to the stars, till then unknown and looked upon as simple points of light, they commenced to discover these stellar groups which had been so long unexplained. The double star γ Virginis was separated in 1718 by Bradley; Castor, α Gemini, in 1719; 61 Cygni in 1753, β Cygni in 1755. Then followed γ Andromedæ, ε Lyræ, 70 p. Ophiuchi, ζ of Cancer, β of the Scorpion, θ and ζ of Orion, ξ of the Great Bear, &c. Their numbers gradually augmented up to the time of Flamsteed, who employed a rudimentary micrometer, until the time of Mayer, who formed a first catalogue of the double stars published in 1756. The philosophical astronomer Lambert, without having himself observed the double stars, published—in his "Photometry" (1760), and in his "Cosmological Letters" (1761)—the first exact notions on the relations of mutual attraction which ought to exist between the components of these partial systems; Lambert thought, with Kepler, that the distant suns ought to be surrounded—like our own sun—with a retinue of obscure stars similar to our planets and comets. As to the stars which were very near to one another, he believed—whilst favouring the idea of an obscure central body—that these stars ought to revolve round their common centre of gravity, and to accomplish their revolution in a sufficiently short space of time. John Michell, who did not know the ideas started by Kant and by Lambert, followed another course (1767). He applied the calculus of probabilities to the study of the stellar groups, and, above all, to the multiple stars. He proved that there were 500,000 chances to 1 that the union of these six principal stars of Pleiades could only be the effect of accident, and that some one cause had been instrumental in determining their approach. He himself became so persuaded of the existence of stars turning round each other, that he proposed the study of these partial systems as a means of solving certain astronomical problems.

Christian Mayer, an astronomer of Manheim, has the great merit of having been the first who seriously observed the double stars (in 1778). This most important branch of his work was not acknowledged until some time after his death.

These trifling but memorable beginnings were followed by the gigantic work of William Herschel, comprehending the long period of more than twenty-five years. The double nature of no star had actually been proved when William Herschel commenced his observations. The discovery of the true double stars is owing to his ingenious perseverance—above all to his observations of the beautiful double star Castor during a period of twenty-five years (1778 to 1803), showing that the relative positions of the two components varied, and that this variation had no relation with the annual translation of the earth. In following this study he was not long in proving many cases of really binary stars, groups in which the little star moved round the larger. "Thus was proclaimed," said Humboldt, "this marvellous truth, that two luminous suns by themselves—and doubtless accompanied each with a planetary system—could turn round their common centre of gravity under the action of the same dynamic laws which govern our solar system." This, then, is a great fact, of which in all probability Newton himself had never thought.

The total number of the systems observed by W. Herschel is 812. His general catalogue was re-edited in 1867, by his son.

These researches were followed, a short time after the death of William Herschel, by observations made by his son, Sir John Herschel, and by his friend James South. They observed together in England, and afterwards separately, James South having come to Paris, and having there fixed his observatory. The first catalogue of these two astronomers contained 360 stars; the second, of South alone, contained 838. These observations were published in the "Philosophical Transactions" for the year 1826.

William Struve afterwards undertook, in Russia, his long and laborious revision of the heavens, for the verification and discovery of all the double stars. His work is composed principally of three folio volumes, written in Latin:—

1. "Catalogus Stellarum Duplicium et Multiplicium Dorpati Dedectarum. Anno 1827 editus."
2. "Mensuræ Micrometricæ Stellarum Duplicium. Anno 1837 editæ, cui accedit additamentum,"
3. "Positiones Mediæ Stellarum Fixarum Duplicium et Multiplicium. St. Petersbourg, 1852."

In making this general review of the heavens, in carrying on his investigations with the stars of the first eight magnitudes, and even on the most brilliant of the ninth, which are comprised between the North Pole and 15° south of the Equator, William Struve observed and catalogued (including Herschel's stars) 3057 double stars.

The celebrated Russian astronomer divided these stars into eight classes, in the order of angular distance, from $0''$ to $32''$. This division is, without doubt, very useful, but it has a defective side, which will cause it to be inevitably abandoned. All classification ought to be made on an invariable base. Nothing is more variable than the angular distance of the components of a double star. Sometimes for the same binary system this distance is less than a second, sometimes it gets beyond a second, sometimes again it gets beyond two or more. Also a star placed in one year in one class will be found in another year belonging to another class. It will be necessary to found this arrangement on mean distances; but these distances are far from accurate determinations. As a methodical division to determine generally the number and separation of systems, the preceding is not valueless, but it is defective as the basis of a catalogue; also there is already difficulty in finding the stars when they are looked for. Thus, for example, the binary system of γ Virginis is not placed in the first class of Struve, because in 1836 the angular distance of its components was inferior to 1″, and it is found in the catalogue of P. Secchi (1860), p. 41, amongst the stars of from 0″ to 1″, although its distance in 1860 was already above 3″. At the present time, in 1875, it is 4·8″. We should thus have to place it, in 1830 in the second class, in 1836 in the first, in 1842 again in the second, in 1850 in the third, in 1870 in the fourth. Many other groups would offer analogous examples. As a system of cataloguing, one by right ascension, or even by constellation, would be evidently preferable.

After the observations of William Struve, we may mention those which Sir John Herschel made during his stay of four years at the Cape of Good Hope, at Feldhausen, where he examined and measured 2100 double stars of the southern hemisphere, of which a few only were before known.

A brilliant series of observations is due to the English astronomer Dawes, who, from 1831 to 1867, made no less than 2762 observations on double stars.

We may finally give a description, in chronological order, of Otto Struve's observations, son of the celebrated William Struve in Russia; of Mädler, in Prussia and in Germany; of Admiral Smyth and Lord Wriottesley, in England; of Captain Jacob, in the Southern hemisphere; of Kayser, at Leyden; of P. Secchi, at Rome; of Powell, in the Southern hemisphere; of Fletcher, in England; of Dembowski, at Gallarate, near Milan; of Brunnow, at Dublin; of Knott and Wilson, in England; of Winlock, at Harvard College (United States); and of Burnham, at Chicago—who, by their researches, their observations, their micrometrical measurements, and their discoveries, have successively endowed astronomy with accumulated riches.

Such are the principal astronomers who have specially devoted themselves to the *observation* of double stars. Others have been occupied with calculations of the orbits of binary systems. The French astronomer Savary was the first who, in 1830, applied a method of calculation to these determinations. Encke afterwards proposed a method much more complicated than that of Savary, which appeared to him to lead to more accurate results. Sir John Herschel, on the contrary, endeavoured afterwards to simplify the calculations, and published, in the "Memoirs of the Astronomical Society of London," a method almost entirely graphic, still leaving, however, much complication in the final formulæ. Successively the astronomers Bessel, Mädler, Hind, Smith, Villarceau, De Gasparis, Wolf of Zurich, and Klinkerfues proposed analytical methods, all of which necessitate the longest and most exact calculations. A little disheartened by these complications, and being convinced that they were not necessary to obtain accurate results (the measurements of double stars not themselves admitting of this precision), I tried, in 1874, to solve these problems more simply, and to find graphically the absolute orbit from the apparent orbit by the assistance of simple trigonometrical formulæ. I have succeeded, and this method is not only incomparably more simple, but even more sure, than the preceding, although it does not pretend to the same degree of precision.[*]

The historical series of these labours shows how science has progressed in less than a century to the profound knowledge of the partial stellar systems, and above all of binary systems.

III. *Nature of Double Stars.*

When a star shows itself double in the field of a telescope, it is not certain on that account that it is a true double star of which the components are associated. The junction observed may be merely an effect of perspective, and one of the two stars may be in reality very far behind the other in space, although found on the same visual ray. In

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[*] My method does not give the period in thousandths of a year, but the principal number is exact. Thus, for example, the preceding methods have given, with pretension to the thousandth of a year (which I abstain from reproducing), the following periods for σ *Coronæ* :—

737	years	Hind.
608	,,	Maedler.
420	,,	Klinkerfues.
240	,,	Powell.
195	,,	Jacob.
843	,,	Darerck.

Evidently of these six periods five at least are imaginary. These calculations —in which the method of least squares plays the principal part—act blindly, and give mechanically divers results, according to the figures which are manipulated. Thus no greater precision must be attached to micrometric results than they deserve. For γ *Virginis* we have results which vary from 629 years to 139. My method gives 175 years, to nearly half a year, and it cannot be wrong. But it cannot be applied to couples for which the measurements are insufficient.

this case there is only an optical group, and the two stars are not physically associated.

To obtain the data necessary to determine the nature of a double star, it must be observed with care during a great number of years, and the relative position of the two components measured as exactly as possible, to ascertain their state and their position. When we possess these precise observations made at intervals of ten, twenty, or fifty years, and concordant intermediate observations, we find either that the two components have remained fixed, stationary, invariable in position one to the other, or else that their relative position has changed, and that they are respectively displaced. In this last case it is necessary to examine carefully the form of the movement. We take the most brilliant of the two components as a point of comparison—we suppose it to be immovable, and we place the second star at the different points observed during the series of years of observation. If these successive positions follow a curve round the principal star, this curve is studied, the missing part is obtained by calculation, and we find that the little star turns round the large one, following an ellipse more or less elongated. This is the apparent orbit as seen from the earth. Often instead of forming a curve, the displacement observed forms a right line. In studying this rectilinear movement, it is found at times that it also resembles an ellipse very much elongated, of which the plane passes through our visual ray, and instead of turning, the secondary star appears to oscillate in a right line from one side to the other of its primary. A wheel which is seen turning in front shows us a circular movement; seen obliquely, it offers an elliptical movement; seen all together from the side, it shows a rectilinear movement, and if it were transparent we should see a mark made on its circumference simply ascend and descend above and below the mean position. The examination of the rectilinear movement shows sometimes, on the contrary, that it is owing to a proper movement of one of the two stars in space, and that the second far distant star rests immovably in the depths of the heavens. There are even many cases where a star passes before two or three others much farther off and immovable. Sometimes these stars pass so exactly one before the other that they eclipse themselves. When they are of different colours, this eclipse of one star by another, so unexpected and so new to science, offers new phases to which no other celestial phenomenon can be compared. There are thus in this world of double stars an immense variety of movements, to which may still be added the effects produced by the sidereal translation of the star system across the changing perspective of the starlit space. It is thus found that there are amongst the double stars couples possessing definite orbital movements, optical couples, due only to the chance of perspective and to our arbitrary position in space, and couples which have remained fixed since their discovery. Do these last represent true systems of stars associated by a physical bond, or are they only perspective groups? Both species can be found among them. When the components of a double star—the whole having preserved the same relative position with regard to each other—are brought together into space by a mutual proper movement, it is thereby evident that they are mutually associated. But do they necessarily turn one round the other, in an extremely slow movement, which may not reach 5° in a century (for this variation would be noticed), and the duration of which may exceed an average of seven thousand years, and may extend, perhaps, to ten, twenty, fifty, or a hundred thousand years? This is probable, at least in the great majority of cases. But although the laws of universal gravitation appear to oppose all other hypotheses, and although astronomers have until lately declared that it must be so, we think that there are cases in which two or more stars might be brought near together by a proper movement in space, without thereby turning round a common centre of gravity situated within their group. This extraordinary fact is proved by the analysis that I have made (among others) of all the observations and of all the measurements taken of 61 *Cygni*. This beautiful double star is one of the most celebrated in the entire heavens, because it is the first of which the terrestrial homunculus had the audacity and the glory of determining the distance. Its parallax being 0·51″, its distance is 403,600 times the distance from here to the sun, that is to say, 14,933,200 millions of leagues. At the rate of 77,000 leagues a second, light would have to fly for six years and five months to cross the abyss which separates us from it. Well! this star is double, and it has been observed as such since 1753, that is to say for 123 years. The illustrious Bessel believed he was able to calculate its orbit, and had estimated its period at 400 years. Since then this period has been raised to 450, 520, and even to 600 years, and its mass has been supposed to be calculated. By a singular chance, it was precisely on the supposed orbit of this famous star that astronomers at first based their arguments on the universality of gravitation; and, moreover, to-day we find it described in all treatises of astronomy as being the most interesting of the orbital systems. But on reuniting the hundreds of micrometrical measurements taken during this long interval of time, I have arrived at the conclusion that the two components of 61 *Cygni* do not turn round each other at all, but that they are carried together in space by a rapid proper movement, which, whilst it is common to both, removes them slowly from each other. These two suns are truly associated, but do not gravitate round each other, and their relative displacement is performed in a right line. This proper movement is one of the most rapid that we know; it is 515″ in a century, a velocity which represents a minimum of *many millions of leagues a day*. These two suns should be one behind the other in perspective, and advance or retire from us in such a way that their line of junction would appear more in face, and their angular distance appear to augment. This system is not the only one of its kind; I have found many others like it. Thus there are stars associated by a proper common movement which do not gravitate round each other.

Amongst the couples of which the components remain not only fixed with regard to each other, but altogether immovable the one to the other at the same point of the celestial sphere, many can form physical couples, and many optical couples. Their distance in the depths of infinity, the slowness of their absolute or relative movement, will only permit us to recognise their nature after centuries of continued observation. Absolute rest does not exist in the immense universe.

Many very distinct causes thus operate on the double stars to give this real or apparent movement: the rotation of the components of a binary, tertiary, or multiple system, round their common centre of gravity; the gravitation of two or more stars carried together in space under the influence of unknown sidereal attractions; the differing proper movements of two stars much separated one behind the other in the line of the visual ray; the secular translation of our solar system in space, which reacts by giving to less distant stars an apparent displacement in a contrary direction.

In giving a general review of the double stars, I have found 614 systems having certain orbital movement, and for each of these I have traced the figure of the observed movement. Thirty couples have travelled over a part of their orbit sufficiently considerable to admit of their orbit being calculated and the period examined. We have even been able not only to measure the light, but also to find the mass of many of these stellar couples, and we thus know that our sun is but one of the smallest stars, and that a certain number of those which shine so modestly under the discreet veil of the silent night are in reality more bulky, more heavy, more resplendent and hotter than this gigantic focus on whose rays the life of our small planet hangs. These groups of suns in orbital movement are connected mutually by the bonds of a reciprocal attraction, and execute their revolutions in closed curves following the laws discovered by Kepler and explained by Newton. Before observation, in our century, had discovered their existence, we only knew similar movements in our solar system, in the translation of the planets round the sun, the satellites round the planets, the periodical comets round the same focus of attraction. We now know that the force which governs our family, which extends with decreasing power as far as the last planet of our system, as far as Neptune, and even twenty-eight times farther, since the solar attraction still governs at 131,000 millions of kilometres the great comet of 1680, retains it in its orbit, and forces it to return, . . . we know, say I, that this force reigns also in other universes, that it is truly universal, and that it governs in the depths of infinity stellar systems the farthest from us.

Stars which, until our day, were called fixed, are, on the contrary, those which move the most rapidly, and the flight of a cannon ball is as the slowness of a tortoise compared to them. The knowledge of these partial systems, whose movements are accomplished without any outward influence, opens to one's thoughts a field so much larger that already these systems appear in their turn like mere details in the vast *ensemble* of movements which animate celestial space.

When one of two associated suns possesses a mass much more powerful than the other, it appears to be the centre of the movement, as our sun appears to be the centre of the movement of translation of the earth and the planets, although in reality the planets and the sun itself turn together round their common centre of gravity, a mathematical point the position of which varies constantly, and which is usually situated in the interior of the sun. The smaller of the two stars revolves round the greater, and no spectacle is more imposing than these sidereal revolutions. In some systems the revolution is made in less than half a century; for example, the couple of stars of the northern crown are two golden suns, of which the cycle is forty years. In some systems the period approaches a century, as in that of 70 *Ophiuchi*, which is composed of a clear yellow sun and a rose sun, which gravitate round each other in a cycle of 92 years. The brilliant couple of γ *Virginis* is composed of two equal suns, which revolve slowly round themselves, and which together turn round their common centre of gravity in a period of 175 years. The tertiary system of ζ of Cancer is composed of three suns: the second turns round the first in a cycle of fifty-eight years, and the third round the two others in six hundred years, describing epicycloidal curves which I discovered at the beginning of the year 1874, and which much perplexed me, as also the astronomers to whom I communicated them at the time. We know, finally, orbital systems, such as those of γ *Leonis*, ϵ *Lyræ*, and of the polar star, of which the cycle surpasses a thousand—and even many thousands—of years. Others move more slowly still. Thus the double stars are so many stellar dials suspended in the heavens, marking, without rest in their majestic silence, the inexorable march of time, which passes there as here, and showing to the earth, from the depths of their unfathomable distances, the years and the centuries of other universes! Eternal clocks in space! Your movement does not stop; your finger, like that of Destiny, points out for other beings the ever-turning wheel, now rising to the height of life, and now plunging into the abyss of death! And we, in our lower sojourn, must read on your perpetual movement the sentence of our terrestrial lot, which will carry along our generation like a grain of dust flying over the paths of heaven, whilst you continue to turn in silence in the mysterious depths of infinity!

IV. *The Number of the Double Stars.*

Double stars are not an exception in Nature; their number, as compared with that of simple stars, is, on the contrary, well worthy of attention; and I only speak here of true multiple stars, not of merely perspective groups. To arrive at an approximate knowledge of this comparative number, I at first examined separately all the stars of the first, second, and third magnitude, and then arranged them in the order of their decreasing brightness, and in each case I noted *its sidereal nature*. The result has been that, of the 300 most brilliant stars of heaven, there are—

		Proportion.
Simple stars	222	0·74
Really multiple stars . . .	78	0·26
	300	1·00

The numerical proportion between the multiple stars and the simple stars is 26 per cent—that is to say, one-fourth of the 300 selected stars.

Is the proportion the same for those of inferior magnitude? To ascertain this I have examined separately 2615 multiple stars described in the great catalogue of Dorpat, and 770 from the catalogue of Pulkowa: they extend from the first to the ninth magnitude. I have found that below the eighth magnitude the progression diminishes, and that to obtain comparable results we ought to stop at the seventh. Deducting those optically double, we find that 2340 physical couples have been noticed in the celestial sphere from the north pole to 150° of southern declination, from the first to the seventh magnitude. In the same extent of the celestial sphere there are the following number of stars:—

1st magnitude	11
2nd ,, ...	34
3rd ,, ...	106
4th ,, ...	325
5th ,, ...	1000
6th ,, ...	3800
7th ,, ...	12500
Total	17776

The proportion of these numbers should give us about two double stars to every fifteen stars. It is certain that this proportion is too low, because the most brilliant stars have been observed more frequently and examined with more care than the less brilliant ones (although at Dorpat and Pulkowa they have scrutinised the heavens with the express aim of finding double stars. Without the perseverance which was devoted to scrutinising so attentively the neighbourhood of Sirius and Procyon, we should not have discovered their satellites. The diminution of the multiple stars below the fourth magnitude may also be due to the increasing feebleness of the satellites, in proportion as the primary ones are themselves smaller or farther off. If we consider the stars of the entire heavens, in order of brightness, we arrive at the following proportion:—

	Number of Stars.	Simple.	Multiple.	Proportion. S.	Proportion. M.
1st magnitude	18	13	5	0·72	0·28
2nd "	59	44	15	0·75	0·25
3rd "	180	136	44	0·76	0·24
4th "	550	435	119	0·79	0·21
	807	628	179	0·76	0·24

The proportion of the multiple systems to the simple stars diminishes gradually with the magnitude, and very probably, as we have just explained, partly on account of the diminution of brightness of the satellites, and partly owing to the less number of observations. We ought, then, to consider that we are very near the truth in taking the average of the first three magnitudes for the general average—that is to say, 74 simple stars and 26 multiple stars in every hundred. Hence we arrive at the following general conclusion :—

The suns, which constitute the fundamental bases of the universe, *are either simple suns*—like that which illuminates us—*or systems composed of two or more suns* associated together by a physical bond. The number of simple suns is greater than that of multiple suns. The proportion between the two appears to be, in round numbers, 3 to 1. One-fourth of the stars of heaven is formed of multiple stars.

If the universe contains 75 millions of stars from the first magnitude to the smallest telescopic magnitude, it is then by *millions* and by tens of millions that we must count the multiple stars. Far from being an exception in nature, they, on the contrary, play a considerable part in the general organisation of the universe.

V. *Multiple and Coloured Suns.*

Our white and solitary sun—our solar system formed of a single focus, round which gravitate obedient worlds following regular orbits—does not constitute the type and model of universal creation. There are multiple suns shining with various hues, sometimes varying their colours, sometimes opposing one another, sometimes successively alternating them in the same sky ; suns of dissimilar volume and mass, acting sometimes in the same direction, sometimes in two contrary directions, to drag out of shape the singular orbits of the unknown worlds which gravitate under their power. No sight is more magnificent than the telescopic contemplation of these strange suns. When during the silent night, during the immense repose of Nature, in these hours of night when the humanity surrounding us is sleeping in anticipated death, our looks and our thoughts rise by means of the marvellous telescope towards these celestial lights which shine on high for other worlds, and radiate around them heat, activity, and life, the contrast is so great that we seem to be in a dream. Here is death, above is light; here is lethargy, above is movement; here is shadow, above is splendour; here is heavy and dull matter, above is devouring flame and sidereal life. How poor is our sun by the side of its grand brothers, of its elders in space! How miserable is our world by the side of those which float above on the rapid and multiplied wings of such an attraction! What delicious hours might contemplative minds and curious souls pass directing a telescope towards the heavens, if the most learned men, if the cleverest women in the world, were not universally ignorant of the most elementary facts of astronomy, and if they did not always live and revolve in the same monotonous circle, without troubling themselves about the marvels that divine Nature holds in reserve for those who comprehend them!

Direct, for example, a telescope towards the beautiful star of the second magnitude, γ Andromedæ. It appears to the naked eye like an ordinary star; but suddenly, to telescopic vision, it shines on the dark background as a double sun, orange and green. Nothing is more striking than this effect. The contrast is splendid, and nothing more beautiful can be imagined. To myself the double star γ Andromedæ, Saturn's ring, and the mountains of the moon in her first quarter, are the three most profound and the most captivating recollections which remain to me of my first observations in astronomy. With a sufficiently powerful telescope we distinguish, at the side of the little emerald-green sun γ Andromedæ, a third—smaller, and of a sapphire-blue colour. We have, then, under our eyes a remarkably curious and wonderful triple star: for a hundred years that the two principal stars here coupled have been observed, they have remained immovable and in the same place with regard to each other, at 63° N. and at 11″ angular distance. The third star was only discovered in 1842, and since that time it has already revolved 26 degrees. If this movement continues on an average, it will revolve round the emerald sun in a period of about 460 years. The orbital movement of the green and blue couple round the orange sun would be incomparably more slow, according to the third law of Kepler (the squares of the time are to each other as the calculated distances—AB = 11″, BC = 0·7), and would only be accomplished in a cycle of many thousands of years. It is possible, then, that the fixedness of the two orange and green suns may be only due to the insufficiency of time for observations; a variation of 3 degrees would be sensible on this couple. The period might extend to twelve thousand years.

One of the most magnificent of the coloured double stars is the star Albires, or β Cygni, topaz and sapphire, not less admirable than the preceding. To the naked eye this is merely a simple star, of the third magnitude. In the telescope it is a celestial couple of great beauty. The large sun shines with a strong and deep yellow colour; the little star appears modestly at its side, of a pensive blue. The contemplation of this ethereal marriage brings to mind the Paradise of Milton, where the poet sings of male and female stars commingling their regards and light across the heavens:—

> . . . . "Other suns, perhaps,
> With their attendant moons thou wilt descry,
> Communicating male and female light."

In the chromatic scale these colours are exactly complementary; moreover, when examined separately, they are real, and not due to an effect of optical contrast. Examined with the spectroscope, the two components of the couple give two different spectra; that of the golden yellow sun is crossed by a series of black absorption lines, occupying its blue part; that of its companion presents an analogous system, occupying its yellow and red region. These stars owe their colour to the nature of the vapours in their atmospheres. Like γ Andromedæ, the couple β Cygni is fixed. It has been observed since 1755, and find the little blue star at 66° 34″ from the topaz star.

Another double star, very beautiful as to its colours, is ε Bouvier, which is called also Pulcherrima, a star of the third magnitude seen with the naked eye, and composed of an orange star and a small brilliant emerald star. The two components are only 3 seconds apart, and it requires a good instrument to separate them. They form an orbital system having a slow movement.

The star α Hercules, also of the third magnitude, may be equally quoted as being one of the most beautiful in the heavens (orange and green). It is stationary at 119° 4″. The brilliant star Antares, of the first magnitude, is red, and this colour can be observed with the naked eye. On a clear and transparent night the attentive eye is sometimes struck by some green rays which furtively mix with its scintillation. In the luminous neighbourhood of this brilliant star has been discovered a little companion completely green, and it is not impossible that sometimes these rays, traversing those of Antares, mingle more or less together. Castor is composed of a yellow star and a second slightly greenish, the reflection of which sometimes modifies the hue of Castor seen by the naked eye.

We may also notice, as types of beautiful coloured stars, the groups of o Eridan (orange and blue), 32 Eridan (yellow, topaz, and greenish blue), 2 of the Triangle (yellow and green), β Scorpionis (yellow and blue), η Persii (red and blue), ξ 163 (orange and sapphire), 35 Pisces (pale yellow and violet), η Cassiopeæ (pale yellow and lilac), δ Orionis (pale yellow and blue), φ in Taurus (red and blue), θ Herculis yellow, green, and purple), α Scorpionis (copper, red, and clear blue), ε Pegasi (golden yellow and azure), γ Dauphinis (cadmium-yellow and variable pearl gray), 95 Herculis (pale green and variable cerise-red), &c. All the colours of the rainbow are represented in the polychromatic illuminations of these far-off suns. Sometimes there are not only two stars which arrest the astonished gaze by the marvellous contrast of their colours, but three, four, five, six, or more; the most admirable example of these masses of multi-coloured stars is situated near the star χ of the southern cross; we can there count not less than 110 stars collected together at the same spot of the heavens, sparkling in all colours, like a casket of precious stones.

The contrast of colours is one of the most remarkable characteristics of the double stars. In 600 couples chosen from Struve's great catalogue, and carefully observed, we find 375 in which the two stars present the same colour and the same degree of intensity; 101 in which the two stars are also of the same colour, but with a difference of intensity: and 120—that is to say, a fifth of the total number—where the colours are completely different. Of these last have been counted 52 where one of the two stars is red and the other very green or very blue; 52 where the first is pale yellow and the second pale blue; and 16 where one is green and the other blue.

In examining the differences of magnitudes of the components in proportion to their differences of colour, we find the following proportions, which are very significant:—The 375 stars of which the colour is the same, with the same intensity, give, for their average difference of magnitude, the number—

0·416.

The 101 stars of which the components are of the same colour, but of different intensities, give for the same difference—

1·144.

And the 120 stars of which the components are of different colours give the number—

1·883.

The disproportion is such that there can remain no doubt that difference of colours is so much stronger in proportion as the difference of the magnitudes is itself greater. The greatest intervals of magnitude are not between the yellow and blue, but between green and blue stars.

Now are all these colours real, or will there not rather be, in most cases, an effect of contrast? A white star placed by the side of a very brilliant red star becomes green by contrast. The retina of the eye, excited by light of one determined colour, becomes insensible to a less intense light of the same colour, and only sees the complementary colour.

Red produces green.
Orange ,, blue.
Yellow ,, violet.
And reciprocally.

To know if these colours are real, and not due to the effect of contrast, it is necessary, when possible, to hide one of the stars while looking at the other; the effect of contrast disappears with its cause, but not immediately, as is generally believed. It takes a certain time—many seconds, and sometimes nearly half a minute—for the eye not to be influenced. It is for that reason difficult to experiment when the two stars of a couple are at a small angular distance. The diurnal movement magnified by the eye-piece quickly causes the star to emerge from behind the wire which has been placed before, even when the telescope has an equatorial movement. But the experiment is easy if we take stars which do not touch. One of the easiest of the examples, even for a moderate instrument, is that of the triple star o^2 Cygni. The large star is of the fourth magnitude; the second, of the 6·5th magnitude, is distant 1′ 47″; and the third, of the fifth magnitude, is at a distance of 5′ 38″. The diversity of the colours is surprising; the first is of a fine yellow, and the other two are blue. If, then, we hide the largest, and look only at the two little ones, they entirely retain their blue colour. The same observation may be made on o^2 Eridan (orange and sky-blue), separated 82″; on β Cygni, of which we have just spoken in reference to the magnificence of their colours, and of which the two components are 34″ apart; an α of the *Hounds* (pale yellow and pale blue), the separation of which is 20″; &c.

Experiment is evidently of use only when the colours are complementary and belong to the two opposite ends of the spectrum—that is to say, where one is yellow, orange, or red, and the other is green, blue, or violet. But if the two are red, or one red and the other yellow,—or if the two are blue, or one green and the other blue,—there is no interfering effect of contrast, and these colours are incontestable.

It is unanimously affirmed (Arago, Humboldt, J. Herschel, &c.) that there is not a single green or blue star by itself in the entire heavens, and that this colour is only met with in the double stars. Having discovered (in my minute and detailed analysis) optical groups in which one of the stars is blue, I have discussed them with the greatest attention, to be sure that these cannot be physical groups. My labours have resulted in the inevitable conclusion that simple blue stars do exist.

Let us quote, for example, the beautiful couple of the double star ζ 2908 Cygni (7th, golden yellow; 8·5th, blue) of complementary but real colours. Their distance apart has augmented from 9″ to 21″ since 1834, and the little blue star has displaced itself in a direction contrary to the proper movement recognised in the large one. In reality the little one is relatively immovable in the sky, and the larger one nearer to us is passing before it. Their distance was at the minimum in 1795. This is only a perspective group, and the two stars are not related. There are, then, simple stars, which are blue.

Among the stars visible to the naked eye I do not know of one which is green or blue. There are white, yellow, and red stars, but none which are of a green colour. β Lyræ has sometimes shown a pale tint; this is the only case which exists, and again it is not authenticated; for whilst many eyes consider it yellow, others see it white, and others somewhat green. The exact estimation of colours is more difficult than is imagined, especially in the case of stars, for their colours are in general faint and transparent. Among the brilliant stars Antares, α Herculis, Mira Ceti, β Pegasi, δ Persii, Pollux, α Orionis, and Aldebaran are red: Arcturus, Castor, the Pole star, Capella, Procyon, β in the Little Bear, α in the Great Bear, are yellow; Sirius, Vega, α Cygni, the cluster in Virgo, Rigulus, Rigel, ε, ζ, η in the

Great Bear, are white. This whiteness is so bright that for certain stars they appear even slightly tinted with blue,

To compare the colour of the stars with each other, and to deduce results applicable to the colouration of the double stars, I have invented an apparatus which forms optical couples with all the simple stars of the firmament, by bringing them as near together as may be desired, and which measures the difference of their tints by the aid of appropriate glasses. This apparatus, to which I have given the name of the Chromascope, and which is founded on the principle of the sextant, has allowed me to compare, one with the other, most of the stars visible above the horizon of Paris, including the planets, employing now and then artificial light to facilitate the comparison. The results which I have obtained are very curious. Thus, for example, a gas-burner seen at 1 kilometre off produces—with Sirius in the telescope—a garnet and sapphire couple; Mars and l'Epi, a red and azure couple; Mars and Saturn, a red and green couple; Saturn and Altair, a yellow and blue couple; the Moon and Sirius, a brass- and silver-coloured couple; Antares and Vega, a red and blue couple; &c.

These comparisons have enabled me to classify the stars in a decreasing order of colour, beginning with red. Here are some examples:—

1. μ of Cephi	Red-orange.
2. Lighting gas	Orange-red.
3. Antares	Orange.
4. Mars	,,
5. Mira Ceti	,,
6. α Herculis	Orange-yellow.
7. Pollux	Yellow-orange.
8. Aldebaran	,,
9. Bételgeuse	,,
10. Polar star	Full yellow.
11. Capella	Yellow.
12. Castor	,,
13. Jupiter	,,
14. Arcturus	,,
15. Procyon	,,
16. The Moon	Creamy yellow.
17. Rigel	Yellowish white.
18. Mercury	White.
19. Sirius	,,
20. L'Epi	,,

21. Vega	Bluish-white.
22. Altair	,,
23. ξ in the Great Bear	,,
24. Saturn	Yellowish-green.

What astonished me most in these researches was to find Mars less red than a gas-flame. I have renewed the experiment more than thirty times before being convinced of this.

By combining the first stars with the last I have formed couples which reproduce, nearly in all their intensity, the double stars red and green, yellow and blue, described above, such as γ Andromedæ, β Cygni, &c. Must we therefore say that it is necessary to suppress all the green, blue, or violet colours of the double stars, and to attribute these colours only to the effect of optical contrast? Undoubtedly no; but we must be sure that these stars are extremely rare, that many of them have their intensity increased by contrast, and that a very large number only owe their hue to the effect of contrast.

In these comparisons it is not only the tint of the stars which produces these contrasts, but the different intensities of the lights play a part which is far from insignificant.

In summing up, therefore, although contrast increases the difference of the colours of the multiple stars, these colours really exist, and all the tints of the spectrum are represented in the illumination of the suns of the universe. But the colours at the red extremity of the spectrum are much more frequent than those at the blue extremity. From the point of view of the effects produced on the unknown worlds which gravitate round these many-coloured suns, we may remark, moreover, that the effects of contrast may augment the real colours, when two or more of these suns appear together above the same horizon. This is what I purpose contemplating in the following chapter.

VI. *Worlds Governed and Illuminated by many Suns.*

Ought we to suppose with a learned contemporary astronomer—M. Faye, Member of the Academy of Sciences, and President of the Bureau des Longitudes—that the idea of the plurality of worlds is a mediocre idea ("Year-Book" for 1874, p. 477); that in all our planetery systems there is only Mars which is habitable; and again (*Id.*, p. 487), that the double and multiple suns are incapable of governing inhabited planets (*Id.*, p. 484); and that for a world to be habitable it is necessary that it should be in just the same astronomical, meteorological, geological, and even geographical condition (*Id.*, p. 485) as the earth which we inhabit? Ought we not, on the contrary, to feel that it would be presumption on our part to impose limits to the power of Nature, and, above all, to take our little planet as the absolute type of creation?—presumption to pretend that the entire universe must be only a vast and useless desert, and that, because we are here collected round our grain of dust, it follows of course that the whole creation is here accumulated? Can we suppose that prolific and inexhaustible Nature, who, long before the appearance of man, peopled our poor planet with beings in climatalogical conditions quite different from those which to-day exist,—who filled the ocean with living beings, contrary to all the deductions of Science previous to the discovery of these beings,—who formed vegetable and animal life with exhaustless profusion on the face of the entire globe, from the polar snows to the heat of the tropics,—and who shows us the earth as all too narrow a field for the life which overflows it all around; ought we to suppose, say I, that this same Nature is everywhere else barren and sterile, and that all the suns of infinity are shining, uselessly spreading round them a light which illuminates *nothing*, a heat which warms *nothing*, an energy which brings forth *nothing*?—attraction, light, heat, magnetism, physical and chemical forces, scattered without object and without effect over the immensity of the universe! When we know that the earth is only an atom in creation, can we pretend that it alone bears on its surface beings who feel, eyes which see, thoughts which elevate them towards Truth, and that the rest of infinity must be void, dumb, and blind? No, a thousand times, *No.* Never shall we come to such a conclusion from the study of Nature; never shall we be led to suppose that our miserable grain of dust constitutes in itself alone all infinity, and contains in this imperfect fragment the whole power of the eternal Creator.

What! Do all these splendid suns which we have been studying exist without an object? Do all these sidereal revolutions which have given them birth, these conflagrations of atoms, these fertilisations of worlds, these appalling geneses, these gigantic displays of power, these systems ruled by immutable laws,—all these powers, all these splendours, have they . . . . only brought forth a mouse? Still less—since the earth does not belong to their system—must we say that all this exists in vain? Must we decree all this to be by us *useless?* Shall we declare all this superfluous, and perhaps even wearisome? We! the atoms of an atom; *we,* blind gropers in the dark; *we,* ignorant dwellers in a cave, who will only believe in the existence of the pale mushrooms which surround us! By what name can designate such pretension? Moreover, this is possibly the reasoning which is indulged in on other planets, where they declare that the earth must be uninhabitable because it is too cold (supposing it to be the inhabitants of Mercury who talk), or because it is too hot (supposing it be the Jovian inhabitants who hold forth). Poor Earth! a microcosm invisible to Jupiter; lost, like a little spot on the sun, to Saturn and the other exterior planets; unknown to all the other solar systems of infinity;—who would believe that it is still declared the queen of creation? The queen, did I say? Who would believe that, as at the time of Joshua, we even now persuade ourselves that we are *alone* in the universe, and that nothing exists beyond our narrow boundaries?

This is not our mode of reasoning. On the contrary, without doubting for a moment that the stars, the suns of space, may be, in general, the centre of planetary systems more or less different from ours, we shall here discuss the novel conditions of worlds governed and illuminated by many suns, of different masses, different volumes, and different colours.

What is the character of the orbits described by the worlds belonging to these singular systems? Do these unknown planets turn round the two suns as their common centre? and have they, as the focus of their movements, the centre of gravity of the twin suns? or, rather, has each of the two suns its proper planetary system? This last should be most probable and most general.

Notwithstanding the essential difference which exists between these systems and ours, we can, however, make use of the disposition of our system to divine the possible arrangement of the others. Already in our system one planet surpasses all the others in its volume, and without doubt also in its intrinsic heat, and it forms the centre of a little system of four moons, which it carries with it during its eleven years' revolution round the sun. Let us suppose that Jupiter, which is already 1400 times larger than the Earth, was of a still more considerable volume, and shone with a blue light; this supposition alone would modify our planetary system to the extent of creating three species of worlds:—First, four globes (the satellites of Jupiter), one of which is larger than the planet Mercury, illuminated and governed by a primary blue sun, and receiving at the same time the more distant illumination of our actual sun; secondly, three immense worlds, Saturn, Uranus, and Neptune, turning round a double sun, one white and the other blue; thirdly, four moderately-sized globes, Mars, the Earth, Venus, and Mercury, turning round the white sun, but illuminated at certain times during the night by a second blue sun. Let us now endow the sun with a red light; we thus reproduce one of the most common types amongst double stars coloured with complementary shades. Let us examine for an instant what a succession of phenomena would be produced by the modification which we have thus made in the sun and in Jupiter.

Let us imagine the Earth as it would be at the time of Jupiter's opposition, that is to say, placed just between the sun and Jupiter. In this position, there would be no night for any point on the Earth; whilst the red sun shone on one side of the Earth, the blue sun would shine on the other; there would thus be a red day on one hemisphere and a blue day at the other, and all the meridians of the globe would pass successively in twenty-four hours through these two kinds of day, distributing to all the countries of the globe twelve hours of red day and twelve hours of blue day, with no night.

But our blue sun would not itself remain stationary in space; it would slowly turn round the red sun; soon it would rise before the other had set, and would appear above the eastern horizon when the celestial ruby had not yet disappeared. The blue day would then succeed, but the sapphire sun not setting in its turn before the rising of its shining rival one, we should have a night for a few moments ornamented with two auroræ boreales of a new kind, the one reddish at the east, the other bluish at the west. The duration of this night would augment from day to day, and at the same time as the double day illuminated by the two suns at a time, the blue hours and the red hours diminishing in the same proportion. Ultimately, and at the epoch corresponding to the conjunction of Jupiter, the blue sun would approach the red sun, and there would be neither red nor blue day, but a double day followed by complete night.

The light of the double day would be formed naturally by the union of the colours of the two suns. It would be violet, but might be quite white, if the colours were complementary. Carried on always by its proper movement, the secondary sun would pass to the west of the first, and would soon produce blue mornings, followed by a white day, or a mixture of a red evening, and a night becoming shorter and shorter, until the blue sun came back to opposition, as it was situated at the commencement of this description.

In most of these systems of double stars, the little star turns round the larger, not in a circle, but describing an elongated ellipse. The stability of the system demands that this little star should not approach too near to the larger one, because, in this case, supposing, as is most likely, that the planets circulate in the same plane as the stars themselves, these planets might be attracted by the central sun at the moment of the perihelion passage, and abandon their primitive sun, to the great detriment of their inhabitants, who would doubtless be burnt up before the astronomers of these worlds had been able to definitely prove the desertion. It is indispensable that these systems be very well restrained round each of the two suns, and that the obedient gravitating planets be kept close under the protecting wing of their own special sun. But in all cases there must be on them the most singular alternation of seasons.

Thus in all planetary systems governed by a double sun, our double alternation of day and night is replaced by a quadruple alternation; 1, a double day lighted by two suns at a time; 2, a single day lighted by a single sun; 3, another single day lighted by the other sun; 4, finally some hours of complete night, when the two suns are together below the horizon.

And the splendour of these natural illuminations can hardly be conceived by our terrestial imagination. The tints which we here admire in these stars can only give a distant idea of the real value of their colours. Already in passing from our misty latitudes to the clear regions of the tropics, the colours of the stars deepen, and the heavens become a veritable casket of precious stones. What would this be if we were transported beyond the limits of our atmosphere? Seen from the moon, these colours should appear splendid. Antares, α Herculis, Pollux, Aldebaran, Bételgeuse, Mars, shining like rubies; the pole star, Capella, Castor, Arcturus, Procyon, would be celestial topazes, whilst Sirius, Vega, and Altair would shine like diamonds, eclipsing all by their dazzling whiteness. What, above all, would this be if we could get near enough to them to distinguish their luminous discs, instead of seeing only brilliant points divested of all diameter.

Blue days, violet days, red days, green days! For us to form an idea of these strange effects, let us mentally change for an instant the colour of our sun, and suppose that instead of the white source of all light which bathes us, a blue light spread over the earth. What a change would instantly come across the face of nature! The clouds losing the silver and golden hues, and spreading under the heavens in a sombre vault; the face of Nature veiled in a coloured penumbra; the most beautiful stars remaining visible by day in the heavens; flowers hiding the brilliancy of their lustrous adornment; the fields bathed in gloom to the invisible horizon; a new day dawns in the heavens; faces become old, and the human race, with astonishment, demands an explanation of so strange a transformation. We know so little, beneath the surface, of things, we go so much by appearances, that the entire universe would seem renewed by this slight modification of solar light.

How would it be if, instead of a single indigo sun following with regularity its apparent course, bringing the years and the days by its single government, a second sun were suddenly to unite itself to it; a blood-red sun disputing always with its partner the empire of the world of colours? Imagine that at midday, when our sun spread over nature the penumbral light we have just described, the fire of a resplendent focus lighted the east with its flames. Greenish shadows pass suddenly across the diffused light; and opposite to every object a dark train obliterates the blue mantle spread over the world. Later, the red sun rises as the other sets, and objects are coloured to the east with red rays, to the west with blue. Later still, a new day dawns upon the Earth, while as the first sun sets, nature becomes bathed in a brilliant fiery red. As night approaches, hardly does west begin to fade like far off Bengal fires in the last rays of the purple sun, when a new morning dawns at the opposite side, tinted with the colours of the Cyclopean blue eye. Can the imagination of poets or the caprice of painters create on the pallet of fancy a world of light more daring than this. Hegel has said, that "what is real can be imagined," and that "what can be imagined is real." This daring thought does not quite express the whole truth. There are many things which do not appear rational to us, and which, nevertheless, exist in reality in the other numberless creations of the infinities which surround us.

There are, no doubt, living eyes there which contemplated these marvels each day. Who knows? It is very probable that they scarcely pay any attention to them, and being from their cradles accustomed, as we are, to the same things, they do not appreciate the picturesque beauty of their lives. In such a manner are we also constituted, the new and the unexpected only affect us. What is natural appears to us to be an eternal necessity, an accident of blind nature, which is not worth the trouble of being observed. If one of those habitants of the far-off world could come to us, whilst they would recognise the simplicity of our little universe, they could not fail to observe with surprise and astonishment our indifference to it.

If like our moon, which gravitates round the globe, and those of Jupiter and Saturn, which together shine on the darkened hemisphere of these worlds, the invisible planets which yonder balance themselves are surrounded by satellites, which accompany them, what ought to be the aspect of these moons lighted by many suns? This moon, which rises behind yonder mountains, has quarters of different colours, at one time red, at another blue; now it is a yellow crescent, and now at full, it is green, and appears suspended in the heavens like an immense fruit. Ruby moons, emerald moons, opal moons! What singular lustre? O nights of Earth, so modestly silvered by our solitary moon, ye are very beautiful when contemplated by the calm and pensive mind! But what is your beauty compared to nights illuminated with those marvellous moons?

And what sort of eclipses of the sun would there be on these worlds? Multiple suns, multiple moons, to what an infinite play of colours would not your mutually eclipsed lights give birth! The blue sun and the yellow sun approach each other, their joint lustre produces green on surfaces illuminated by both, and yellow or blue on those which only receive one light. Soon the yellow sun draws near the blue, it eats into its disc, and the green, spread on the pale world, fades and fades till it dies, melting in the golden hue which pours its crystalline rays in space. A total eclipse colours the world yellow! An annular eclipse shows a blue ring round a gold medal. By degrees, insensibly, the green shines out, and, recovering its empire . . . . . Let us add to this phenomenon that which will be produced if a moon, coming in the middle of this gilded eclipse, covers the yellow sun himself and plunges the world in obscurity; then, following the relation existing between its movement and that of the sun, it continues to hide it after the emergence of blue disc, and then leaves nature to repose again under the obscurity of an azure veil! Let us still further suppose . . . . . but no, we have encroached upon the boundless treasury of nature, remaining inexhaustible in spite of all our efforts.

VII. *Stellar Systems, of which the Orbits are Calculated.*

Amongst the thousands of multiple stars studied, there are only a very small number of which the observations are, on the one hand, sufficiently numerous, and the movements sufficiently rapid, in the next place, for it to be possible to determine the elements of their orbits. In order that our readers may understand the nature and the varieties of these orbits, we collect together here the systems which have, to the present time, been able to be calculated, writing them in order, increasing with their periods. As a rule, each of these orbits has been calculated by many astronomers, and the results have shown considerable divergence. The elements which we publish here are the last calculated. The periods which are not absolutely sure are marked thus (±)—.

DOUBLE STARS WHOSE ORBITS HAVE BEEN CALCULATED.

Names.		Magnitude.	Half-Major Axis.	Periods. Years.	Colours.	Calculators.
42 ϐ Capella		6—6	0·66″	25·71	White	O. Struve, 1875
ζ Hercules		3—6	1·19	34·57	Reddish yellow	Flammarion, '73
η Northern Crown ..		5·5—6	0·86	40·17	Yellow	Flammarion, '74
ζ in Cancer (tertiary system)	AB	5·5—7	0·91	58·80	White	Flammarion, '74
	AC	5·5—6	5·00	700 ±	White & yell.	
ξ Great Bear		4—5	2·45	60·60	Golden yellow & ashy	Flammarion, '73
α in Centaur		1—2	15·50	77·00	White	Powell, 1870
70 p. Ophiuchi		4·9—6	4·78	92·77	Yellow and rose	Flammarion, '74
ξ in Scorpion		5—5·3	1·20	98·00	White	Flammarion, '75
γ Austral Corona.. ..		6—6	2·55	100·00	White	Capt. Jacob, '58
ω of the Lion		9—7	1·05	136·00	Yellow	Klinkerfues, 1870
3062 ζ Cassiopeia.. ..		7—8	1·00	147 ±	Yellow and olive	Maelder, 1850
ξ of Bouvier..		3—6	9·95	169 ±	Yellow and orange	Hind, 1872
γ Virgo		3—3	3·38	175·00	Golden yellow	Flammarion, '74
η Cassiopeia		4—7·5	10·68	176·00	Yellow and lilac	Duner, 1875
δ Cygni		3—8	1·80	178·00	Yellow & violet, variable	Hind, 1864
τ Ophiuchi		5—6	1·11	185·00	White	Doberck, 1875
36 Ophiuchi		4—6	4·00	200 ±	Red and yellow	Admiral Smyth, 1860
1757 ζ Virgo		8—9	2·00	240 ±	White and yellow	Admiral Smyth, 1860

Names.	Magnitude.	Half-Major Axis.	Periods. Years.	Colours.	Calculators.
λ Ophiuchi	4—5·5	1·20	250 ±	White and ashy	Capt. Jacob, '58
μ^2 in Bouvier	8—8·5	1·50	290·00	Green	Doberck, 1875
36 Andromedæ	6—7	1·54	349·00	Orange and yellow	Doberck, 1875
γ Lion	2—4	2·00	402·00	Yellow	Doberck, 1875
ε of θ Hydra	3—4	3·00	490 ±	Yellow and purple	Admiral Smyth. 1860
μ in the Dragon	4—4·5	3·00	600 ±	White	Hind, 1865
49 in the Serpent	6—6·5	3·00	600 ±	White and yellow	Klinkerfues, 1870
12^2 in Lynx	6—6·5	1·50	700 ±	White and red	Maelder, 1850
γ Coronæ	6—6·5	6·00	843·00	White & ashy	Doberck, 1875
Castor	2·3—3	7·74	996·00	Yellow	Thiele, 1872
ε^1 Lyræ	5—6·5	3·00	1000 ±	Yellow and orange	Maelder, 1850
ξ in Versian	4—4·5	7·64	1578·00	White	Doberck, 1875
ε^2 Lyræ	5—5·5	2·90	2000 ±	White	Maelder, 1850.

We see in this table that there is a great variety of periods, and although those which are inscribed here extend over 2000 years, there are much longer periods which are still only vaguely estimated. We may remark that the colours of the stars in rapid orbital movement do not offer the beautiful contrasts of which we were speaking before. This is a surprising fact, and one which has greatly astonished me; *the most beautifully-coloured stars turn only very slowly round each other.* There is not a single contrast in the thirty-one systems of this table; *α* Cygni only seems to offer an exception; but its colours are undecided and variable. One can remark, also, that amongst these movements some are direct, that is to say accomplishing in the direction east, south, west, north, and whilst others go backward (west, south, east, and north), one of the orbits, that of ξ Scorpionis, is very inclined on our visual ray; this makes the orbit appear very elongated. Another, that of 42 Capella, lies entirely in the plain of the visual ray, so that we only see a waving movement of it. As can be easily verified, the greatest variety reigns in these stellar systems.

VIII. *Masses of Double Stars—How we Weigh a Star.*

The double stars are not only immense suns shining with their proper light in the depths of the heavens, but we know now that they are heavy, bulky, ponderous, and incomparably heavier than the earth on which we are, heavier even than our colossal sun.

The power which we have of weighing a star is, unquestionably, one of the most surprising results of the progress of science, one of those which leaves the greatest doubt in the mind of persons unaccustomed to the principles of celestial mechanism. To weigh a star is a work even more extraordinary than to measure its distance, and neither Copernicus, Galileo, Kepler, nor Newton certainly ever dreamt the day was coming when their successors would be capable, by the application of their immortal discoveries, to determine the mass of a star lost in the depths of celestial space. Therefore, we think we ought to complete this summary statement of our actual knowledge of the double stars by indicating the results arrived at on the magnitude and masses of these systems, and in giving an idea of the method employed to obtain them.

The mass of a star is calculated by the energy of the action which it exercises around itself. If the earth was ten times heavier than it is, preserving always the same volume, it would attract bodies towards its surface ten times more strongly than it does now, and an object which in falling travels over 16 feet in the first second of falling would travel 160 feet. If the Earth, whilst preserving its volume, had the mass of the sun, it would attract bodies 324,000 times stronger; and an object which now weighs 1 kilog. would weigh 324,000. A man of the average weight of 70 kilog., would weigh 22 millions! We can measure the weight of a star by the intensity of the attraction at its surface. Reduced to its simplest expression in its application to the fall of bodies, this attraction would be difficult to verify; but we can determine it by the speed of a satellite revolving round the star of which we wish to know the mass.

For example, the attraction of the earth has the power of curving the right line, which would be followed by the moon in space if it did not follow this attraction; and it curves by attracting it in such a manner that it makes a circumference travelled over in 27 days 7 hours 43 minutes. If the mass or energy of the earth were to augment, the velocity of the moon in its orbit would increase also: if it were to diminish, the contrary effect would be produced. The attraction varies in direct proportion with the masses. The quickness of the moon's movement round the earth comes from the actual force of the earth. The earth is the hand which makes the moon turn in opposition. If the earth had more power, more energy than it has, it would make the moon turn quicker, and *vice versa.* The same occurs in respect to the sun and the earth. If the sun were increased in weight, the earth and the other planets would turn more quickly round him, and the years would diminish in length. If the mass of the sun were diminished, the contrary would happen. It is by comparing the action of the sun on the earth with the action of the earth on the moon, that we have found that the sun is 324,000 times more energetic, more powerful, more heavy than the earth.

If, then, we had in space a celestial couple, the mutual distance of the two components of which was equal to that which separates the earth from the sun, or 92 millions of miles, the examination of the duration of its revolution would immediately give us the mass of the system relatively to that of the sun. It is easy to show that if a couple of celestial bodies turning round their common centre of gravity employs a certain time, T, to accomplish its revolution, whilst another couple, of which the components are at the same distance from each other, employs another duration of time, D, to accomplish its revolution, the mass of the first is to the mass of the second in the inverse proportion to the square of the time, that is to say as D^2 is to T^2.

If the distance is not the same, it is necessary at first to reduce it to this equality, by bearing in mind the law which regulates the distances. "The squares of the times are to each other as the cubes of the distances."

I have been able to calculate the mass of the stellar system of the double star 70 p. Ophiuchi. By the combination of all the observations, I have found that the period is 92·77 years, or 92 years 283 days. The parallax of this star being 0·168″ corresponds to a distance from the earth equal to 1,400,000 times that of the sun. At this immense distance, the size of the terrestrial orbit being reduced to the preceding angle, the half major axis of 4·88″ represents 1075 millions of leagues. This is a little less than the distance from Neptune to the sun. A planet situated at this distance from the sun will accomplish its revolution in 156·55 years. The proportion between them is then as $(156{\cdot}55)^2$ is to $(92{\cdot}77)^2$, or as 2·89 is to 1, whence I have concluded that *the mass of the Ophiuchus system is nearly three times superior to that of the sun* and Neptune unitedly, or (Neptune having only a small mass) to that of the sun alone. Thus there is a star, scarcely visible to the naked eye, which weighs 900,000 times more than the Earth.

Let us remark, in passing, that the orbital movement of the small star round the large one is 191,830 leagues a day, or 8881 metres a second, and that these two twin suns travel through immensity with a speed of which the minimum is 246 millions of leagues a year! And these are the stars which were but yesterday called *fixed stars!*

Calculations made on the other stars have led to equivalent results, showing us these celestial torches as gigantic and heavy suns, the enormous distance which separates us from them reducing them to simple mathematical points. The star nearest to us, α Centauri, has a parallel of 0·91″, and, in consequence, its distance from the earth is about 8 trillions 376 millions of leagues. If we adopt 15·5″ for the average value of the angle comprised between the two components, we find that their distance apart is 2520 millions of trilometres, or 630 millions of leagues; that is, less than the distance between Uranus and the sun. Its period appears to be about 77 years. The result is that it weighs a little less than our sun, and that in representing its mass by ten, that of the sun should be represented by twelve. But its volume ought to be greater (we always speak of the two stars united), because its intrinsic light is about three times superior to that of our sun. If we were to consider the quantity of light as a criterion of the surface of emission, we should find that the diameter surpasses that of the sun in the proportion of seventeen to ten.

The period of Cassiopeia, which at first was estimated at 700 years, then at 181 years, has now been fixed at 176, and it is probable that this number is not very far from the truth. In admitting the parallax to be 0·154″, which removes it from us 55 trillions of leagues, and the semi-major axis to be 10·68″, the mass of this system will be ten times that of the sun.

As to the mass of 61 Cygni, which we find described in all astronomical works, it is wrong, and, as we have already seen, it is impossible to determine, considering that the two components do not turn round each other.

Thus the double stars are truly suns, gigantic and powerful, governing in the regions of space illuminated by their splendour systems of worlds different from those of which we form a part. The sky is no longer a gloomy desert; its antique solitudes have given place to regions peopled like that in which the earth gravitates; the obscurity, the silence, the death which reigned in these heights has given place to light, to movement, and to life; the thousands and millions of suns pour lavishly into space energy, heat, and the diverse undulations which emanate from them as a focus; all these movements succeed each other, cross each other, fight against each other, or unite in the incessant maintenance and the development of universal life; the universe is transfigured to our minds; suns succeed to suns, worlds to worlds, universes to universes; formidable movements carry all these systems through the boundless regions of immensity; and everywhere to the farthest boundary where fatigued imagination can rest its wings, everywhere developing in its infinite variety, we meet with divine creation, of which our microscopical planet is only an imperceptible province.

V. ON THE COLOURING OF THE SHELLS OF BIRDS' EGGS.

By H. C. Sorby, F.R.S., &c., Pres. R.M.S.

ONE of the interesting results due to the spectrum method of research, as applied to various biological subjects, is that there is far greater unity and simplicity in the elementary colouring matters than might have been expected from the very various tints of the natural objects. Their number is indeed very considerable, and in some cases, though the colour may be nearly the same, the substances may be most materially different. Very often, however, we meet with an almost endless variety of tints, due not to any essential difference in the elementary colouring matters, but to the varying proportions of a few well-marked substances. This is very well illustrated by the colour of many eggs—even, perhaps, of the same species of bird—which may, for example, pass from greenish through all shades of brown to reddish, brought about by the varying development of a blue and of a red constituent; or may, in other eggs, pass from blue to very yellow green, owing to a variation in the relative amounts of a blue and of a yellow colouring matter.

Another interesting fact connected with this subject is that there is often a close and remarkable relation between different classes of objects and their characteristic coloured constituents. This is strikingly the case with the more important of those met with in the shells of birds' eggs, since, so far, I have never met with them in any other natural products. Further research may, indeed, lead to their discovery elsewhere; but, at all events, birds' feathers of very similar colour contain entirely different substances. Much remains to be done before the subject can be looked upon as fully understood; but, still, I have examined sufficiently numerous examples to be able to form a tolerably good general opinion. The detail would require much further research. A paper on the subject, published in the "Proceedings of the Zoological Society of London," May 4, 1875, p. 351, gave a full account of what I had been able to learn up to that date, and since then my attention has been directed to entirely different subjects, so that I am unable to add much to what I then said, and can merely speak with greater confidence respecting some closely allied subjects. On the whole, I cannot do better than make some extracts from this paper, with any further illustrations that may appear desirable.

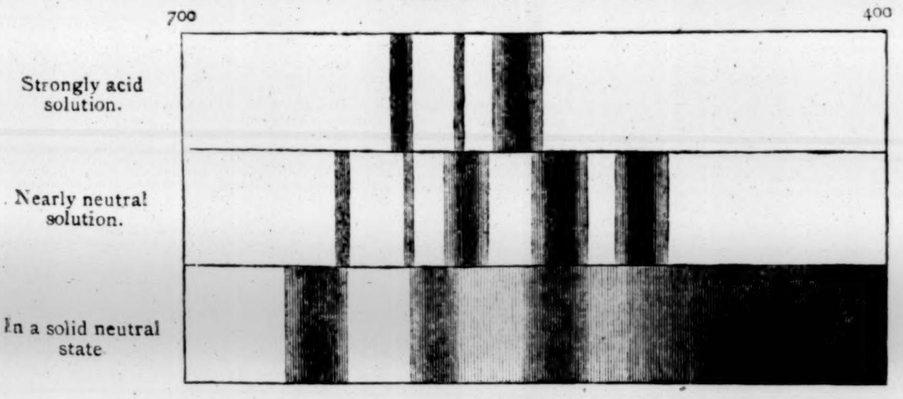
Hitherto I have been able to distinguish eight well-marked substances. One of these is identical with a colouring-matter met with in nearly all groups of plants, from the lowest to the highest; another is probably the black pigment found in feathers; but I have not yet been able to identify any of the rest with any found elsewhere. But, at the same time, I must admit that our knowledge of animal colouring-matters is far too limited to make such negative evidence of much value. All these seven coloured substances found in the shells of birds' eggs are insoluble in water, but soluble in absolute alcohol, either when neutral or when a small amount of free acid is present. They are also sometimes soluble in chloroform or carbon bisulphide. Neutral or acid absolute alcohol, however, is in every respect the most convenient and best solvent. Some are extremely permanent, and resist the action of powerful reagents, whereas others are of such unstable character that they are not only rapidly changed by acids or oxidising reagents, but are even partially decomposed by evaporating their solutions to a dryness at a gentle heat. In these general peculiarities they resemble bile-pigments more than any other group of colouring-matters, but do not actually agree with any that have passed under my notice. Some of them furnish us with a number of most interesting facts in illustration of the probable existence of a connection between optical properties and chemical or molecular constitution; but on the present occasion I forbear to enter into such questions, and will confine myself as much as possible to the zoological aspect of the subject. At the same time, it is absolutely necessary to enter into a certain amount of chemical and optical details, since otherwise the characteristic peculiarities of the different substances could not be established.

Some important and reliable information may be learned from the spectrum of the light reflected from the eggs themselves or transmitted through broken fragments; but in order to study colouring-matters in a satisfactory manner, it is requisite to obtain them in solution, so that they may be more or less separated from one another, their spectra seen to greater advantage, and the effect of various reagents determined. In the shells of eggs the coloured substances are so intimately associated with carbonate of lime that they cannot be dissolved out; and even when this has been removed, they are often so firmly enclosed in other insoluble organic substances, that it is difficult or impossible to dissolve them out completely. In the majority of cases it is best to remove the earthy carbonates by means of somewhat dilute hydrochloric acid, added gradually until no further effervescence takes place. The character of the residue varies much in different cases. Sometimes we obtain a coloured membrane, occasionally like dark morocco leather, whilst in other cases the membranous part is very pale, and the colour chiefly occurs in detached skin-like flocks, or as minute particles disseminated through the liquid; collecting this insoluble portion on a filter, washing well with water, and removing any large portions of colourless membrane and of the filter to which no colouring matter is attached, the coloured residue should be freed from superfluous water, but not actually dried, and placed whilst moist in absolute alcohol. This usually dissolves out a considerable amount of the colour; but some still remains insoluble. A portion of this is occasionally soluble in alcohol containing free acetic acid; but very often much remains undissolved until the residue is treated with alcohol containing hydrochloric acid. Sometimes even this fails to remove all, even when heated for many hours. All these different solutions should be kept separate, since they usually differ most materially; and in no case should a strong acid solvent be used unless found to be necessary, because several of the normal colouring-matters are rapidly decomposed by strong free acids. For this reason it is in some cases advisable to separate the carbonate of lime from the shell by means of acetic acid; but then unfortunately the colouring-matters are much less readily dissolved out of the residue by alcohol.

These general remarks will, I trust, suffice to indicate the character of the methods usually employed; and I therefore now proceed to describe the different coloured substances hitherto met with. The number of species of eggs which I have been able to carefully study is less than I could have wished; but what I have already been able to learn suffices to explain the more obvious peculiarities of the colours of eggs, since those examined were carefully selected for this purpose.

1. *Oorhodeine.*—This is perhaps the most important and interesting of all the colouring-matters, not only because it gives a number of most interesting spectra, of such a well-marked character that a very minute quantity can be recognised without any difficulty, even when mixed with a relatively large quantity of coloured impurities, but because it occurs, in large or small amount, in the shells of such a great number of eggs that its entire absence is exceptional. When in a perfectly neutral condition it is almost insoluble in alcohol; so that when the washed shell-residue is digested in cold absolute alcohol very little is dissolved, until a small quantity of hydrochloric acid is added. On evaporating this solution to dryness at a gentle heat, and treating it at once with absolute alcohol, a considerable part dissolves, probably because a small quantity of acid clings to it; but if a small excess of ammonia be added, and the solution again evaporated to dryness, the neutral residue is all but insoluble in alcohol. These peculiarities enable us to separate oorhodeine from most of the other colouring-matters, and to obtain it approximately pure. It gives spectra with extremely well-marked absorption-bands, which differ in number, character, and position according to the conditions in which it occurs. The more important of these spectra are shown by fig. 1, in which, as well as in all the other figures, they are given, not as seen with a *prism*, but as they would appear in an *interference* spectroscope, since in that case alone do we see the true relations of the different parts.* To any one

FIG. 1.



Spectra of Oorhodeine.

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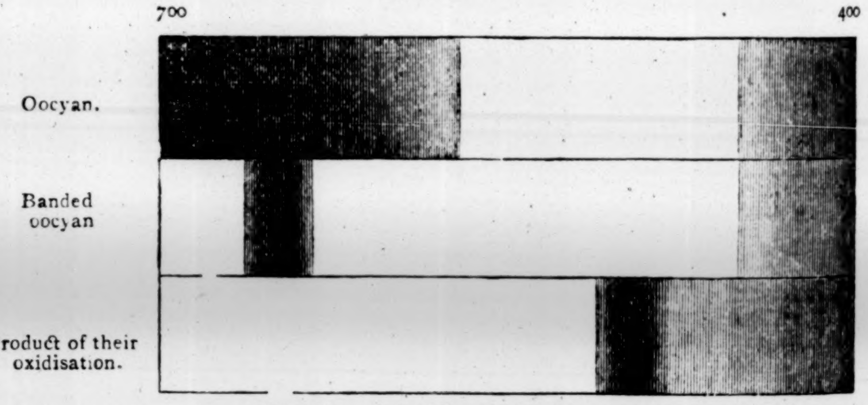
* We are indebted to the kindness of the Zoological Society for the use of the woodcuts illustrating this paper.

accustomed only to an ordinary spectroscope the blue end will therefore appear abnormally contracted and the red end expanded unusually. The numbers given at the top represent millionths of a millimetre of wave-lengths of the light.

As will be seen, the strongly acid solution gives a spectrum with three bands, two of which are so well marked that a most minute quantity of the substance serves to show them in a satisfactory manner. When as small a quantity of free acid is present as will enable the alcohol to dissolve oorhodeine, the spectrum shows the five absorption-bands given in fig. 1, and the general colour is brownish-red. This in the spectrum of the almost neutral modification when in a state of *solution*. When in a free *solid* form, as in the shell, or as found in the washed dry skin-like residue after removing the carbonate of lime by an acid, the spectrum is most materially different, as will be seen from the woodcut. Only three bands are distinctly visible, and they lie nearer to the red end, whilst there is far more of general absorption at the blue end. The result of this is that the general colour is a peculiar brown-red.

FIG. 2.



Spectra of the Oocyans, &c.

Oorhodeine is of such a very permanent character that it resists the action of very powerful reagents. I have been able to destroy it, but have not yet succeeded in changing it into any other coloured substance.

2. *Oocyan.*—In most cases this is readily soluble in neutral alcohol, and can thus be separated from oorhodeine. It is, however, often associated with yellow substances that cannot easily be removed; very commonly, therefore, the solution is of a somewhat green-blue colour, but in many cases the yellow impurity is far more easily decomposed by the action of light or by weak oxidising reagents, and can be removed by this means, so as to enable us to determine the true

colour and spectrum of the oocyan itself. When dissolved in alcohol it is of a very fine blue colour. The spectrum shows no detached bands, but a strong general absorption of the entire red end and of a small portion of the extreme blue, as shown in fig. 2.

3. *Banded Oocyan.*—This also is of fine blue colour, but differs from the former species in giving a spectrum with a well-marked detached absorption-band near the red end, as shown in fig. 2. It is also far less soluble in neutral alcohol, so that it is left in the shell-residue after having been digested for some time in cold neutral alcohol, and can subsequently be dissolved out by alcohol, to which a minute quantity of hydrochloric acid has been added. The solution must, however, be examined at once, since banded oocyan is rapidly decomposed by strong acids.

Both these different kinds of blue colouring-matter are evidently in a state of very unstable equilibrium. Sometimes the greater part of the colour is lost by merely evaporating their solutions to dryness at a gentle heat; and several very interesting products can easily be obtained by acting on them with reagents.

On adding a moderate excess of hydrochloric acid to a solution of oocyan, no other immediate change occurs than the destruction of some of the yellow substances that may be present; but in the case of *banded* oocyan, two new faint bands are developed in the orange and yellow end of the green, and it is gradually changed into a new modification, or perhaps even into a new substance, characterised by giving a spectrum with two bands, quite unlike that of the original. On adding to the solution of banded oocyan a little hydrochloric acid and potassic nitrite, it is rapidly decomposed into an orange-coloured substance, giving a spectrum with a simple well-marked absorption-band between the green and blue, as shown in the figure. In the case of oocyan this same substance is also produced: but there is an intermediate red compound formed, characterised by giving a spectrum with two bands (one in the orange, and the other at the yellow end of the green), which, however, do not correspond to those of the product of the action of acid on the banded oocyan.

It will thus be seen that these two blue colouring-matters (oocyan and banded oocyan) differ in very important particulars, but are obviously closely related, since they both yield the same well-marked product when oxidised.

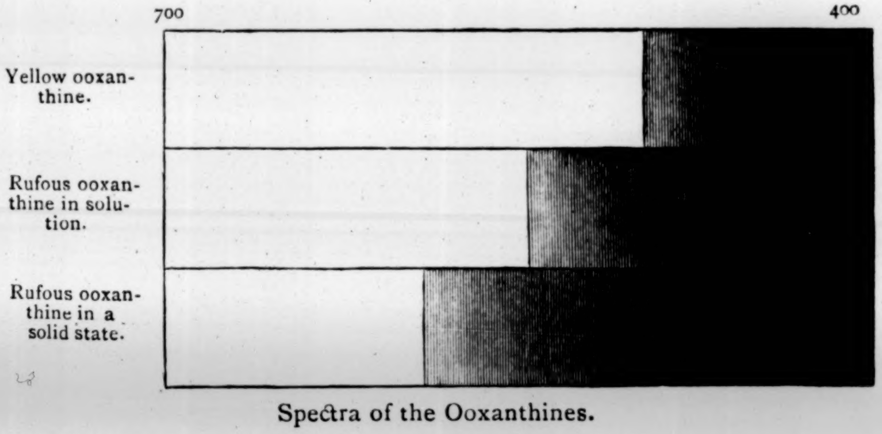
4. *Yellow Ooxanthine.*—This substance may be best obtained from moderately fresh Emu-eggs. These are of a

fine malachite green colour, due to a mixture of yellow ooxanthine with oocyan. On completely dissolving out the carbonate of lime with moderately strong hydrochloric acid, the residue is of deep green-blue colour, and a large part of the ooxanthine is decomposed by the action of the acid. On the contrary, if the carbonate of lime be dissolved out by acetic acid, nearly all the oocyan is lost, and a yellow residue is obtained, coloured by yellow ooxanthine, which, however, is so firmly associated with the thick tough membrane, that it is almost impossible to dissolve it out in alcohol. If, however, the shell be partially dissolved in dilute hydrochloric acid, a yellow layer is formed on the surface, which may be detached from the greener part below, not yet free from the earthy matter; and this yellow layer easily gives up part of its colour to neutral alcohol, and a further quantity to alcohol containing a little acetic acid. These solutions are of a clear yellow colour, giving a spectrum with no detached bands, absorbing the whole of the blue light, and strongly transmitting nearly all the green and the whole of the red end of the spectrum; that is to say, light of less wave-length than 500 millionths of a millimetre is absorbed, and of greater wave-length transmitted. In a solid state, in the egg-shell, the absorption extends down to wave-length 508. Alkalies and weak acids produce no immediate change in the solution; but a strong acid like hydrochloric rapidly decomposes yellow ooxanthine, and leaves only a pale, almost colourless residue of another substance, which will be described in the sequel. This change takes place immediately if a minute portion of potassic nitrite be added to the acid solution. The alcoholic neutral or acetic solution is also rapidly decolorised by exposure to direct sunlight. Hence it will be seen that this yellow substance is in a state of very unstable equilibrium, and is rapidly decomposed by oxidisation, when a strong acid is present in a free state, or when exposed to bright light.

5. *Rufous Ooxanthine.*—Hitherto I have not met with this substance in any other eggs but those of the different species of Tinamou, and have studied it more especially in those of *Rhynchotis rufescens*, in which it occurs associated with much oocyan. It agrees with yellow ooxanthine in being rapidly decomposed by a strong free acid, and immediately when a little potassic nitrite is added: but it is not so easily, if indeed at all, destroyed by the action of a moderately dilute aqueous solution of hydrochloric acid; and its presence does not seem to have any effect in decomposing the oocyan; whereas yellow ooxanthine has a most remarkable influence,

since, as will be apparent from what I have already said, when the carbonate of lime is dissolved out by a weak acid the whole of the oocyan disappears if the amount of yellow ooxanthine is considerable, whereas no such decomposition occurs when it is absent. Rufous ooxanthine also differs from yellow ooxanthine in absorbing light to a very considerably greater distance from the blue end. Even when dissolved in alcohol it absorbs not only all the blue, but also at least one-half of the green; that is to say, all light of less wave-length than 550 millionths of a millimetre is absorbed, and all of greater wave-length transmitted, which, of course, is a very well-marked difference, as will be seen on comparing the spectra given in fig. 3.

FIG. 3.



Spectra of the Ooxanthines.

When in a solid state in the egg the absorption extends considerably further towards the red end, down to wave-length 590 or thereabouts; so that the tint is decidedly red, and not the orange-colour of the solution or the bright yellow of yellow ooxanthine. When mixed with oocyan, it therefore gives rise to the peculiar lead-colour of the eggs of the rufous Tinamou—and not to green, like that of the fresh eggs of the Emu.

6. *Substance giving narrow absorption-bands in the red.*—Unfortunately I have not yet succeeded in obtaining this in sufficient quantity, or sufficiently free from other substances, to be able to decide whether its true colour is blue, green, or brown; but the fact of its giving a spectrum with several narrow absorption-bands in the red would certainly indicate that, when mixed with other colouring-matters, it would cause them to have an abnormally browner tint. Small quantities of it occur in very many eggs; but I have not yet found it so abundant in any as to exercise a more important

effect on the general colour than to make it somewhat more dull. Since the entire spectrum is not accurately known, I will merely give the position of the different very narrow absorption-bands in millionths of millimetres of wave-length. The most complete spectrum shows three bands. On adding excess of ammonia, that nearest the red end alone remains, whilst the addition of a small excess of a strong acid removes all but the central band—and when the excess is considerable, raises this band towards the blue end. These facts will be better shown by the following table:—

	Centre of bands.		
Most complete spectrum . . .	668	648	628
With excess of ammonia . . .	668	—	—
With a little acid	—	643	—
With much acid	—	641	—

By means of these bands a very small quantity of this substance can easily be recognised. It is not readily decomposed—but, when acted upon with oxidising reagents, may be changed into another colouring-matter, giving rise to a spectrum with one or two somewhat obscure bands.

7. *Lichnoxanthine.*—In my paper on comparative vegetal chromatology, I have described a substance which occurs in greater or less amount in almost all classes of plants, from the lowest to the highest, but is more especially abundant in and characteristic of lichens and fungi, and for this reason has been named by me *lichnoxanthine.* The spectrum shows strong general absorption of the blue end down to about wave-length 510 millionths of a millimetre, and a much weaker general absorption down to about 590 millionths. Acids and alkalies produce very little change; and it is very slowly altered by strong oxidising reagents. I have been able to prepare it artificially by the decomposition of resins. Some such substance is undoubtedly present in small quantity in very many kinds of birds' eggs; and occasionally there is so much as to materially modify the general colour. It may occasionally have been, to some extent, derived from the decayed vegetable matter of the nest, or, in the case of eggs which have been kept long, may be partly due to the growth of minute fungi; but, at the same time, a very closely allied, if not identical, substance does really appear to be a normal constituent of the shell of eggs having a peculiar brick-red colour. Very probably it may not be formed in the animal organism, but absorbed directly from the vegetable food of the birds. This need not in any way surprise us, since it is a substance of great stability, and there is strong

reason to suspect that many yellow feathers are coloured by the far less stable xanthophylls derived from that source. The loose yellow pigment on the beak of the hornbill, with which he paints his feathers and makes himself look a far smarter bird than he otherwise would be, corresponds in every particular with a mixture of xanthophyll with lichnoxanthine and a little yellow xanthophyll, all of which could be so readily absorbed from the vegetable food along with fatty or waxy substances, that it seems unnecessary to suppose that they are formed in the bird's own organism, though this is of course quite possible.

Such, then, is a general account of those peculiarities of the colouring matters that have come under my notice, which suffice to distinguish them from one another and from analogous substances met with elsewhere; and I now proceed to a more detailed consideration of the eggs themselves. As an illustration of the method of study, suppose that we have taken portions of the brownish-red eggs of the common Grouse, of the pure brown eggs of the Nightingale, and of the pure blue of the common Thrush, separated from the black spots, kept for examination by themselves. After having, in each case, dissolved out the carbonate of lime with dilute hydrochloric acid and having washed the residues with water, they should each be digested in cold neutral absolute alcohol. Scarcely any colour would be dissolved out in the case of the Grouse—but a fine blue in all the others, which, on further examination, would be found to be oocyan, with mere traces of other substances. After having dissolved out as much as possible, by means of fresh neutral alcohol, the residue should be digested in alcohol with a small quantity of hydrochloric acid. It would then be found that the Grouse-shell would give a rose-coloured solution, containing much of the acid modification of oorhodeine. The Nightingale would also give much oorhodeine, but the colour would be modified by the presence of oocyan; the blue portion of the Thrush-egg would give a small quantity of a fine blue substance, showing the spectrum of banded oocyan, with little or no trace of oorhodeine, whereas the dark spots would be found to give a very considerable quantity of oorhodeine. We thus clearly see that the redder egg is mainly coloured with oorhodeine; the blue egg with oocyan—the brown colour of the Nightingale being due to a mixture of these two, and the black spots on the Thrush-egg to patches containing much oorhodeine. All the various intermediate shades of colour, passing from red through brown to blue, whether they occur in the eggs of different species or in the more or less variable eggs of the same kind of bird, or in patches on the same egg, can thus be explained without any difficulty.

In a similar manner the various shades of green, passing from the blue-green of such eggs as those of the common Hedge Sparrow to the fine malachite green of the fresh Emu, and to the very yellow-green seen on them in patches, are all due to a variable mixture of oocyan with yellow ooxanthine.

As is no doubt well known, many green eggs turn blue on long keeping. In this manner the beautiful malachite green of fresh Emu-eggs passes into dark blue. This is easily explained by the fact that yellow ooxanthine is much more easily destroyed by oxidisation than oocyan. A portion of a green Emu egg exposed to strong light soon becomes much bluer, and so does a mixed solution of the two colouring-matters in alcohol, the yellow constituent being destroyed and the blue left.

A few eggs are of a brick-red colour. Those of Cetti's Warbler are as good an example as any I have seen ; and on carefully comparing them with the browner red egg of the common Grouse, I found that both contained a large amount of oorhodeine, but that the tint was made more dull in the case of the Grouse by the presence of a small quantity of the colouring-matter which gives the narrow bands in the red ; whereas in the case of Cetti's Warbler this was almost or quite absent, and there was present a relatively very unusually large amount of the orange-coloured substance, which I have not been able to distinguish from lichnoxanthine. To the presence of this substance we may thus attribute the brick-red tints seen in a few eggs.

The eggs of the black variety of the common Duck are coloured with a nearly black substance, which I have not yet obtained in a state of solution, and probably corresponds to the so-called *pigmentum nigrum*, which may be obtained in larger quantity in a more satisfactory condition from black hairs or feathers, and used for drawings as a splendid black pigment, quite equal to the best Indian ink.

My studies of colouring-matters by the spectrum method soon led me to perceive that the individual species of certain groups of coloured substances are so intimately connected with their life that plants may be arranged in a kind of natural order according to the presence, absence, or relative proportion of the various coloured constituents, which order on the whole agrees remarkably with that founded on structural characters, as shown in my paper on comparative vegetal chromatology.* There is also reason to suspect a connection between the evolution of one colouring-matter from another, or from colourless constituents, and the progress of the general organisation. These facts naturally led me to consider whether any such connection could be recognised in the case of birds' eggs. Much remains to be learned before any positive opinion can be expressed; but what is already known appears to be sufficient to prove that, if there be any definite connection between the general organisation of birds and the coloured substances found in their eggs, it is not of such a kind as is at all obvious to any one who, like myself, is not thoroughly acquainted with anatomical details. Six out of the eight different colouring-matters occur in variable amount in a very great variety of eggs, but there is no greater variation than is met with in the different individual eggs of the common Guillemot; so that the study of the colouring-matters cannot be looked upon as of any value in distinguishing species, or even much wider groups, except, perhaps, in one particular instance. Hitherto I have met with rufous ooxanthine only in the eggs of the Tinamous, and perhaps in those of some species of Cassowary; and though the question needs further examination, it is desirable to give a short account of what is already known.

As previously described, rufous ooxanthine when in solid form in the shell of such redder Tinamou-eggs as those of *Crypturus obsoletus* (Wickham), absorbs the blue, the green, and some of the yellow rays, but transmits the orange and red; so that the colour is a sort of orange-red, made duller and of more leaden tint in the eggs of other species by mixture with oocyan. The result is that we obtain tints which are not so decidedly different from those due to a mixture of oocyan with oorhodeine as to lead any one to conclude at once that they were not due to the same substances. However, when the eggs in their natural state are properly illuminated by light so condensed on them sideways from a lamp that as little as possible is reflected from the surface, the spectra are seen to differ entirely. When oorhodeine is present, one or more of its absorption-bands may be seen; but when the red colour is due to rufous ooxanthine, no trace of any such bands can be recognised. My knowledge of the chemical and optical characters of rufous ooxanthine when in a state of solution were derived from the study of the eggs of the rufous Tinamou (*Rhynchotis rufescens*); and hitherto I have been able to study only the spectra of the eggs of other species through the kindness of Mr. Osbert Salvin, on whose authority I give the various names. Taking all the facts of the case into consideration, it appears to me to be almost certain that the redder-coloured constituent in all the different species is rufous ooxanthine. At all events, none show any trace of the bands of oorhodeine, and all show the same absorption of light of less wave-length than about 590 millionths of a millimetre. All that remains to be done to make this point certain is to examine the *solutions* derived from other species than that I have named, in order to be sure that the chemical as well as the optical characters are identical. In the present state of the question the following conclusions must be looked upon as only extremely probable.

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*Proceedings of the Royal Society, vol. xxi., p. 442.

No species of Tinamou yet examined contains any recognisable amount of oorhodeine. The colour of many species is due to a variable mixture of rufous ooxanthine with oocyan, the former greatly preponderating in such red eggs as those of *Crypturus obsoletus*, *C. pileatus*, and *Nothoprocta curvirostris*. The red and blue constituents occur in more equal proportion in the peculiar lead-coloured eggs of *Rhynchotis rufescens*. *Calodromas elegans*, when in a comparatively fresh state, contains so much yellow ooxanthine that it is pale green yellow; but by exposure to light this yellow constituent is decomposed, and the shell becomes a pale flesh-colour from the small residual amount of rufous ooxanthine. Fresh-laid eggs of *Tinamus solitarius* are of nearly the same deep green as those of the Emu; and the long-kept eggs of *Tinamus robustus* are of fine blue, as though in some species there were very little rufous ooxanthine, and the colouring, as in the case of the Emu, due to a mixture of oocyan and yellow ooxanthine. It will thus be seen that all the various peculiar tints can be explained by the presence of a variable quantity of rufous ooxanthine.

I have carefully examined the spectra of many other eggs which appeared at all likely to contain rufous ooxanthine, but have not yet seen any facts which seem to indicate that it occurs in any other group of birds than the Tinamous, unless, indeed, it be in the case of the eggs of *Casuarius bennettii* and *C. australis*. If further examination should confirm these conclusions, it appears to me that the facts will be of much interest in connection with comparative physiology, as showing that, to a limited extent, even in the case of birds' eggs, there is a connection between the general organisation of the animals and their coloured secretions, since, as will be seen, such a well-marked group of birds as the Tinamous appears to be equally well distinguished by the formation of a special colouring-matter in the egg.

Connection between the Colouring-matters of Eggs and other Organic Products.

It would obviously be very interesting to learn what connection there is between the various colouring-matters described in this paper and substances met with elsewhere. Perhaps further inquiry may lead to the discovery of some of them in other situations; but, with the exception of lichnoxanthine and the pigmentum nigrum, I have not been able to identify them with confidence except in the shells of birds' eggs. The spectra of oorhodeine are so well marked that there could be no difficulty in recognising a comparatively small quantity; and yet no trace can be detected in feathers whose general colour is practically identical with that of birds' eggs coloured with oorhodeine.

In considering the relation between the coloured substances in birds' eggs and other natural or artificial products, we are at once brought face to face with a branch of inquiry which seems to promise most valuable results, but is now so much in its infancy that the conclusions can only be looked upon as very plausible. In a paper recently read before the Royal Microscopical Society* I have shown that in some cases it is certain, and in others probable, that when a coloured constituent is common to a number of distinctly different compounds these may and do generally give spectra which are most intimately related in the *ratio* of the *wave-lengths* of the centres of their absorption-bands, but the *actual* wave-lengths differ in the different spectra. We may perhaps better understand the facts by supposing that when a substance combined with the coloured constituent is replaced by some other, the general *shape* and constitution of the ultimate molecules is so slightly changed that the general character of the spectrum is the same, but the *size* of the molecules so far altered that they are put into relation with waves of light of a different length. It appears to me, therefore, that when we meet with two substances which give almost exactly the same spectra and are changed in the same manner by the addition of reagents—in fact differ from one another only in the *numerical values*, and not in the *relations* of the wave-lengths of the bands or in any other essential particular—we may look upon it as very probable that there is some important chemical or physical relation between the two compounds.

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* Monthly Microscopical Journal, vol. xiii. p. 198.

Since writing the above-named paper I have met with a number of facts which make me more and more convinced of the truth of this supposition. I have found several excellent cases in which spectra are closely alike in every particular except in the exact wave-length of the centres of the bands, and though the two substances differ most materially in other characters, it is easy to prove that they are very closely related, and perhaps yield the same product when decomposed by the action of certain reagents. The difference of a few millionths of a millimetre in the wave-length of the bands, which a superficial observer might overlook, may thus serve to teach us far more important facts than if the spectra had been entirely different; and is a good illustration of the importance of minute accuracy in scientific investigation, and of looking upon nothing as accidental and insignificant.

A striking example of such a relation is the connection between the spectra of oorhodeine and of the product of the decomposition of the red colouring-matter of blood by strong sulphuric acid, discovered and described by Thudicum.* When in a nearly neutral state, dissolved in alcohol, it gives a spectrum of exactly the same character as that of oorhodeine in the same physical condition; and on adding a small quantity of a strong acid to both they are both changed in the same manner, and give new spectra which are also of exactly the same character. The spectra of their neutral modifications and also those of the very acid solutions, have most remarkable and unusual peculiarities, quite unlike those of any other substances; and therefore one cannot, I think, attribute the resemblance to mere accident. The agreement is so close that a superficial observer might easily be led to conclude that they were absolutely identical, and that oorhodeine was merely Thudicum's cruentine; but when the spectra are compared together side by side with a suitable instrument, it may be seen that although the number, relative intensity, and relative position of the bands, both in a neutral and acid condition, are exactly the same, the *position* of the band is *not* the same. The difference between the spectra is exactly like what is so often seen on comparing together the spectra of the same substance dissolved in different liquids; but this explanation will not apply in this case, because I find that the position of the bands in cruentine does *not vary* with the nature of the solvent, and the difference between its spectra and those of oorhodeine occurs when they are both dissolved in the *same* solvent. It is more analogous to the connection between the two kinds of hæmoglobin, described in my paper on the evolution of that substance, published in the current number of the "Quarterly Journal of Microscopical Science," which can be proved to contain the same hæmatin united with a different albumenoid.

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* Tenth Report of the Medical Officer of the Privy Council, p. 227.

In order to show the nature of the relation of the spectra, I subjoin the following tables, giving the position of the centres of the absorption-bands in millionths of a millimetre of wave-length.

Table 1.—*Dissolved in nearly neutral Alcohol.*

Oorhodeine	630	602	578	539	504
Cruentine	623	596	572	534	500
Difference . .	7	6	6	5	4

Table 2.—*Dissolved in Alcohol with strong Acid.*

Oorhodeine . . .	604	580	557
Cruentine	598	574	552
Difference . .	6	6	5

Though I feel much tempted to enter further into the purely physical part of the question, it will, I think, be better to confine myself to what bears more directly on zoological facts. According, then, to the above-described general principles, these facts lead us to conclude that oorhodeine is in some way or other closely related to cruentine, but not identical with it, as shown not only by the well-marked difference in the spectra, but also by the difference in their solubility and power of resisting the decomposing action of powerful reagents.

In the present state of our knowledge the most plausible explanation of all the facts is that perhaps oorhodeine and cruentine contain some common coloured radical of the same chemical or physical constitution, combined with some other substance which is itself colourless, and that this second constituent is not the same in oorhodeine as in cruentine, but differs sufficiently to modify the general properties and to slightly alter the size of the ultimate molecules, so as to cause them to be related to waves of light of a little different length. Assuming this principle to be true, the facts lead to the conclusion that the oorhodeine of birds' eggs is derived from the red colouring-matter of the blood, not by any mere mechanical exudation, but by some unknown physiological process of secretion, which breaks up the highly complex molecule of hæmoglobin into one which can be formed artificially by heating it with strong sulphuric acid; but in the living organism it combines with a second substance differing from that with which it combines when the change is effected by the action of hot strong sulphuric acid. Whether this view of the subject be in all respects true or not, it at all events appears to me very plausible and well worthy of further examination, as pointing to the source of one of the most important colouring-matters of birds' eggs.

Relations of the Oocyans.

In their normal condition the fæces of man, and probably those of many other animals, contain a yellow colouring-matter, which by oxidisation yields a substance closely related to, if not identical with, a product of the oxidisation of the bilirubin of bile described by Jaffé[*] and by Heynsius and Campbell.[†] When extracted from fæces by alcohol without contact with the air, it gives a spectrum which cuts off the blue end without any definite band; but when exposed to the air, or treated with some oxidising reagent, the solution becomes orange-coloured, and the spectrum shows a well-marked, dark, moderately broad absorption-band between the blue and the green, having its centre at wave-length 495 millionths of a millimetre. The addition of an excess of ammonia immediately removes this band without producing any well-marked change in the colour. Now I find, on comparing this substance with the product of the oxidisation of the two species of oocyan, which gives the spectrum shown by Fig. 2, that there is a close agreement in general characters, but yet a well-marked difference. The band in the product from the oocyans is about 5-4ths the breadth; and its centre is a little farther from the blue end, being at wave-length 497; and caustic potash does not develop any band as in the other substances. On the whole, then, if we follow the same line of reasoning as that adopted in the case of oorhodeine, we are led to conclude that the product of the oxidisation of the two kinds of oocyan is in some way connected with a product of the change and oxidisation of the colouring-matter of bile; and thus we may perhaps be justified in concluding that there is some chemical relation between the oocyans and bile. Bilirubin can indeed easily be converted by oxidisation into a blue substance; but this differs entirely from either of the oocyans, both in its spectrum and in the character of the products of its decomposition. The residual bile-product found in fæces is in all probability a representative of a much further stage of change than to the oocyans; and if it could give rise to them it would be by a process of integration, which is not at all likely. On the whole their connection with bile is as if we had two parallel series of products depending on two distinct physiological processes—one in the liver giving bile, and the other in the oviduct giving rise to egg-shell pigments.

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[*] VIRCHOW'S Archiv., vol. xlvii., p. 262.

[†] PFLUGER'S Archiv., vol. iv., p. 520.

The application of the various methods and general principles described in this paper furnishes us with a very wide field for inquiry. It may safely be said that scarcely anything is yet known compared with what remains to be learned. The very foundations of the science require to be laid, and the full significance of the various facts determined, since optical facts have outstripped chemical knowledge. I however trust that the general outline I have given of this particular branch of inquiry will serve to indicate what kind of results we may hope to obtain, and to show that such investigations may throw an unexpected light on many interesting biological problems.

VI. THE EARLIEST MEDICAL WORK EXTANT.

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REFERRING to my "Outlines of a Bibliography of the History of Chemistry,"* Mr. G. F. Rodwell, author of "The Birth of Chemistry," has expressed the hope that in the progress of Egyptian discovery valuable information in regard to the history of chemistry may be brought to light. This hope has been in some measure realised by the appearance of a fac-simile of an Egyptian medical treatise written in the 16th century B.C., which when fully deciphered will undoubtedly prove of immense value to the historian and to the student of science. Though strictly a medical work, it reveals much relating to ancient

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* "The Chemical News," vol. xxxii., pp. 36, 56, 68.

Egyptian domestic life, and is said to be the largest, best preserved, and most legible text in the language of hieroglyphics.

The title translated is as follows :—

Papyrus Ebers, the Hermetic Book of Medicines of the Ancient Egyptians, in Hieratic Writing. Published, with Synopsis of Contents and Introduction, by George Ebers. With a Hieroglyphic-Latin Glossary by Ludwig Stern. Under the Patronage of the Royal Bureau of Education in Saxony. Leipzig: William Engelmann, 1875. 2 vols. Folio.

The papyrus of which this work is a fac-simile reproduction was discovered by the archæologist Ebers, during his visit to Egypt in the winter of 1872-3. Ebers and his friend Stern were residing at Thebes, collecting archæological data, and there became acquainted with a well-to-do Arab from Luxor, who brought to them for sale a wooden image of Osiris and a papyrus of no especial value. Suspecting that the Arab was holding in reserve objects of greater interest, Ebers offered him a considerable sum for any superior specimens in his possession. This induced the Arab to return on the following day, bringing with him a metallic case containing a papyrus roll enveloped in mummy cloths. Ebers immediately perceived he had a prize, but was unable to command the large sum of money demanded for it until provided with the means through the liberality of a German gentleman, Max Günther, travelling in that vicinity. According to the Arab's account, the papyrus had been discovered fourteen years previously, by a man since dead, between the bones of a mummy in a tomb of the Theban Necropolis.

Ebers hastened back to Leipzig with his precious roll, and deposited it for safe keeping in the University Library of that city. And now, with the co-operation of an enterprising publisher and the assistance of royal patronage, he places it at the disposal of the civilised world, by reproducing it in these handsome volumes.

The papyrus as received by Ebers consisted of a single solidly-rolled sheet of yellow-brown papyrus of finest quality, three-tenths of a metre wide, and a little more than twenty metres long. It formed one enormous book, but was divided into 110 pages, which were carefully numbered. For purposes of preservation and exhibition in convenient form it has since been cut into several lengths. The writing, which is exceedingly clear and regular, is partly in black and partly in red ink, the latter occurring at the heads of sections and in the expression of weights and measures. The characters are known as Hieratic, being a cursive form of the hieroglyphic method of writing, and bearing the same relation to the latter that our ordinary written hand does to printed characters. Hieratic script resulted from attempts to simplify the forms and outlines of the ideographic characters employed in hieroglyphic writing, which is essentially a combination of picture-writing with a phonetic system. Hieroglyphics in ancient Egypt were the written language of the people, and Hieratic writing was chiefly confined to the sacerdotal caste.

The Papyrus Ebers is so marvellously well preserved that not a single letter is lacking in the entire roll. The material of the papyrus itself, the inner bark of *Cyperus papyrus*, was examined by Professor Schenck, Professor of Botany in the University of Leipzig, who established its identity with that of similar rolls, and pronounced it of remarkably good manufacture.

The age of the manuscript was determined by a consideration of three points:—1. Palæographic studies of the form of the written characters; 2. Occurrences of names of Kings; 3. Examination of a calendar which occurs on the back of the first page. These data enable Ebers to assign the writing to the middle of the 16th century, or more precisely 1552 B.C. Accepting this date—and it has been established beyond reasonable doubt—the writing was prior to the exodus of the Israelites: in fact, according to the commonly received chronology, Moses, in 1552 B.C., was just 21 years of age.

The authorship of this ancient treatise is not revealed, but it bears internal evidence of being one of the six Hermetic Books on Medicine named by Clement of Alexandria (200 A.D.) The Egyptian priests, who were also the physicians, in order to give greater authority to their writings, were wont to ascribe them to their gods, and their codified medical knowledge was generally ascribed to the god Thuti (or Thoth). In proof of this, Ebers quotes the following passage from page 1, lines 8 and 9, of the papyrus in question:—

"Ra pities the sick; his Teacher is Thuti, who gives him speech, who makes this book and gives the instruction to scholars and to physicians in their succession."

This god Thuti, also written Thoth and Taant, is the famous Hermes Trismegistus of the Greeks, the same who was regarded by the alchemists of the Middle Ages with superstitious reverence as the Father of Alchemy. However this may be, historians accord in representing Hermes as the inventor of arts and sciences, and that he first taught the Egyptians writing, invented arithmetic, geometry, astronomy, and music; gave laws to the people, and regulated their religious ceremonies. At the time of Jamblichus, A.D. 363, the priests of Egypt showed 42 books which they attributed to Hermes (Thuti). Of these, according to that author, 36 contained the history of all human knowledge; the last six treated of anatomy, of disease, of affections of the eye, instruments of surgery, and of medicines. The Papyrus Ebers is indisputably one of these ancient Hermetic works. A study of the synopsis of the contents given in part below will justify this belief.

The recipes and prescriptions contained in this treatise are evidently collected from various sources, some of them being quoted from still more ancient writings. It bears internal evidence of having been used in the healing art, for the word "good" occurs in the margin in several places, written in a different handwriting from the body of the work and with lighter coloured ink. Ebers thinks the compilation was made by the College of Priests at Thebes, basing his conjecture partly on the locality in which it was discovered.

Ebers gives a synopsis of the contents of the entire work and a literal translation of the first two pages of the roll, reserving a fuller translation with commentary for a future publication. A hieroglyphic translation of a portion of the Hieratic manuscript also accompanies the plates; the latter, 107 in number, are faithful and beautiful reproductions of the original papyrus in the same yellow-brown colour. The second volume contains a hieroglyphic-Latin glossary by Stern and the remainder of the plates. Before proceeding to give details of its contents, one more peculiarity is worth mentioning. Though the pages are carefully numbered, the figures 28 and 29 are omitted, while the text is continuous. Ebers conjectures that the writer either accidentally forgot his count or abstained from using these numbers for superstitious reasons.

As already remarked, the work is divided into chapters or sections. A fair insight into the character of the treatise may be obtained from the selected headings of sections and extracts here following:—

Headings of selected chapters. The numbers refer to the pages of the papyrus:

1. Of the preparation of medicines.
25. Of salves for removing the *uhan*.
47. Catalogue of the various uses of the *Tequem* tree.

48. Medicines for alleviating the accumulation of urine and diseases of the abdomen.

55. The book of the eyes.

65. Medicaments for preventing the hair turning grey and for the treatment of the hair.

66. Medicines for forcing the growth of the hair.

79. Salves for strengthening the nerves and medicines for healing the nerves.

85. Medicine for curing diseases of the tongue.

89. Medicines for the removal of lice and fleas.

91. Medicines for ears hard of hearing.

99. The Secret Book of the Physician. The science of the beating of the heart and the knowledge of the heart as taught by the priestly physician Nebsecht.

Verily " There is no new thing under the sun." Chapters 65, 66, 79, and 89 show that hair invigorators, hair dyes, pain-killers, and flea powders were desiderata 3400 years ago.

Ebers encountered immense difficulties in the work of deciphering this papyrus on account of the large number of technical terms; as an example of the obstacles met, he gives the following literal translation of a diagnosis beginning on plate xxxvi., line 4:—

" Rules for the *re-het*, that is, suffering in the pit of the stomach (pylorus or cardia.) When thou findest anybody with a hardening of his *re-het*, and when eating he feels a pressure in his bowels (*chet*), his stomach (*het*) is swollen, and he feels ill while walking, like one who is suffering from heat in the back, *tau nu peht*, then look at him when he is lying outstretched, and if thou findest his bowels hot and a hardening in his *re-het*, then say to thyself this is a liver complaint, *sepu pu n merest*. Then make thyself a remedy according to the secrets in botanical knowledge from the plant *pa chestet* and from scraps of dates. Mix it and put it in water. The patient may drink it on four mornings to purge his body. If after that thou findest both sides of his bowels (*chet*), namely, the right one hot and the left one cool, then say of it: That is bile. Look at him again, and if you find his bowels entirely cold, then say to thyself: His liver, (?) *merest*, is cleansed and purified; he has taken the medicine, *sep nef sep*, the medicine has taken effect."

In view of the direction to look at the patient " when lying outstretched, " it is curious to note that (according to Dunglison) the priestly physicians of Egypt are said by Diodorus to have formed their diagnosis principally on the position which the patient assumed in bed.

The following is the translation of the first four lines of Plate I.:—

"The book begins with the preparation of the medicines for all portions of the body of a patient. I came from Heliopolis with the Great Ones from *Het aat*, the Lords of Protection, the masters of Eternity and Salvation. I came from Sais with the Mother-goddesses, who extended to me protection. The Lord of the Universe told me how to free the gods from all murderous diseases."

The work abounds in prescriptions, of which the following are samples:—

Beginning of the Book of Medicines. To remove illness from the stomach. Rub up the seed of the *Thehui* plant with vinegar and give the patient to drink.

The same for sick bowels.

Caraway seed	1-64 dram.
Goose fat	1-8 dram.
Milk	1 tenat.

Boil, stir, and eat.

The same:

Pomegranate seed	1-8 dram.
Sycamore fruit (?)	1-8 dram.
Beer	1 tenat.

Treat as above.

In the original the arrangement of the substances and quantities in two columns is the same as here given. The weights are written in red ink. Other prescriptions contain reference to pills made by mixing certain substances with honey and rolling them into little balls. The weights and measures in this unique work deserve a longer notice than space will permit. Certain characters with dots above them represent weights, and a series of special signs indicate measures of volume. The unit of weight employed is believed by Ebers to bear a close relation to the later Arabic dirhem, or dram, which is equivalent to about 48 English grains. But owing to the smallness of the quantities given in the prescriptions, the unit is probably double the drachm in value. This unit is represented in hieroglyphics by a spindle-shaped figure, and divisions of this unit into eighths, sixteenths, thirty-seconds, and sixty-fourths were indicated by arbitrary dots and other characters placed beneath the sign of the unit, in certain positions; the fractions 1-8, 1-16, 1-32, 1-64 always recurring, and

1-16 predominating; a quarternary arrangement which was superstitiously regarded as beneficial.

The unit of volume is thought to be the *tenat*, which is equivalent to six-tenths of a litre. This unit and its subdivisions are represented in the hieratic script by arbitrary signs. When equal parts of the components of a prescription are taken, the fact is indicated by a light short vertical dash placed opposite each substance.

Ebers states in his preface that notwithstanding there are to be found in this wonderful work many incantations and conjurations from which the priestly physicians could not abstain, still there is no hocus pocus nor gibberish in it. On the contrary, it shows that it was possible to write in the sixteenth century B. C. complex recipes, and that they understood how to administer with care the medicines prescribed. Moreover, sorcery was forbidden in the ancient times in the strongest manner, and the alchemistic magi were punished in the reign of Rameses III. with death. The art of the physician was lost in the post-Christian era; science became more and more tinged with magic, and was gradually obscured and degraded by it.

NOTICES OF BOOKS.

The Movements and Habits of Climbing Plants. By CHARLES DARWIN, F.R.S. Second Edition. London: Murray.

EVEN in the woodlands and hedge-rows of so-called temperate regions, climbing plants form a striking feature. Their graceful forms, the ease with which they are adapted to decorative purposes, and a certain weird character, which they seem to share with the serpent tribe, appeal at once to our æsthetic and imaginative faculties. But on a closer scrutiny we find that they afford at least an equal scope for the spirit of scientific inquiry. The first point which strikes even the cursory observer is the diversity of means by which the common object of deriving support from other plants or from inanimate substances is attained. We see plants with long flexible shoots which merely scramble over and through bushes, supporting themselves by their side-twigs, their leaves, or their prickles. Familiar instances of this may be found in brambles. These scramblers stand on the debateable land of climbing plants. If support is to be found they accept it in a rough way. But if not, they form independent bushes. Then we find root-climbers, of which the ivy may serve as an illustration. From its twigs it sends out broad, flattened roots, which attach themselves to every crevice and irregularity of the surface up which the ivy is climbing. Hence it is admirably adapted for covering the faces of rocks, walls, or the trunks of thick trees. Next come the true twiners, which twist spirally around any object which they are able to grasp, but are quite unfit to cling to a flat surface. The hop and the common bindweed, as well as its garden congener the "morning glory," are types of this class. Lastly, we have plants which, like the vine and the pea, climb by means of special organs for laying hold of any suitable object. The next point which must have struck every observer even slightly versed in plant-lore is that climbers do not form or belong to any one botanical order or group of orders, but appear scattered through the whole vegetable kingdom.

Such, we may say, was the state of popular knowledge on the subject when Mr. Darwin entered upon the researches which form the matter of the work before us. He does not, however, profess to be the first scientific investigator of the phenomena presented by climbing plants. When his observations were more than half completed he learned that the "spontaneous revolutions of the stems and tendrils of climbing plants had been observed by Palm and Hugo von Mohl" as far back as 1827, and had been again investigated by Dutrochet in 1843. We may here remark that the difficulty of finding whether a given subject has been already investigated is greater in Natural History than perhaps in any other branch of science. Important observations in zoology and botany may be found, not merely in the majority of scientific and literary periodicals, but even in sporting and political organs.

It must not, however, be supposed that Mr. Darwin's sole merit in this matter consists in verifying previous researches and in presenting them in a form accessible to the English reader. He tells us, with perfect justice :—" I believe that my observations, founded on the examination of above a hundred widely distinct living species, contain sufficient novelty to justify me in publishing them."

Twining plants form, it appears, the largest subdivision of the climbers, and represent, according to Mr. Darwin, the primordial and simplest condition of the class. If a young hop-shoot be observed as it rises from the ground, the first two or three joints are straight, and remain stationary, like the shoot of a non-climbing tree. The next joint, however, when still quite young, bends to one side, and moves slowly round to all points of the compass, travelling with the sun. In hot weather, if the plant is in vigorous health, each revolution is completed in two hours and about eight minutes. As the plant grows up the older joints lose this property, but the three top joints always continue to rotate. If the shoot is left free it describes a circle of about 19 inches in diameter. Another twiner, the *Ceropegia Gardnerii*, revolves in a direction opposite to the sun, and describes a circle of 62 inches in diameter. When one of these revolving shoots encounters a stick it twines round it in a spiral form. The thickness of the object found by a shoot is a very material point. The common nightshade (*Solanum dulcamara*) can twine only around such stems as are at once thin and flexible. The only native English twiner which can clasp trees is the honeysuckle, which Mr. Darwin has found twining up a young beech tree 4½ inches in diameter. In a room lighted on one side *Phaseolus multiflorus* could not ascend posts of from 3 to 4 inches in diameter. In the open air it could twine round supports of this thickness, but failed ascending one of 9 inches. In South Brazil F. Müller saw a tree about 5 feet in circumference spirally ascended by a plant belonging to the Menispermaceæ. Mr. Darwin very aptly remarks that in cold climates it would be "injurious to the twining plants which die down every year if they were enabled to twine round trunks of trees, for they could not grow tall enough in a single season to reach the summit and gain the light." Twining plants with very long revolving shoots are not necessarily able to ascend thick supports, their great length and power of movement merely aiding them in finding a distant object up which to climb. The rate of revolution in all the plants observed by Mr. Darwin was merely the same by day as by night, whence he infers that the action of the light is confined to retarding one semicircle and accelerating the other, not greatly modifying the speed of the whole revolution. It has actually been found that in *Ipomœa jucunda* the semi-circle from the light takes 5½ hours, whilst the semi-circle towards the light is effected in 1 hour. In most twiners the branches, however numerous they may be, all go on revolving together. In *Tamus elephantipes* only the side branches twine and not the main stem. In a certain climbing asparagus the case was reversed. A plant of *Combretum argenteum* made a number of short, healthy shoots, which showed no signs of revolving, but at last it put out from the lower part of one of its main branches a thin sheet, 5 or 6 feet in length, which revolved vigorously and climbed. *Polygonum convulvulus*, according to Palm, twines only during the middle of the summer, but in autumn, even if growing vigorously, shows no tendency to climb. Three vegetable species, —two *Ceropegias* and *Ipomœa argyrœoides*—in their dry home in South Africa grow erect and compact, but seedlings raised near Dublin, presumably in a conservatory, twined up sticks from 6 to 8 feet in height. On these significant facts Mr. Darwin thus comments:—"There can hardly be a doubt that in the drier provinces of South Africa these plants have propagated themselves for thousands of generations in an erect condition: and yet they have retained during this whole period the innate power of spontaneously revolving and twining whenever their shoots become elongated under proper conditions of life."

But we must now turn from the twiners to those plants which climb by means of prehensile organs, possessing a certain sensibility or irritability. The simplest and least developed of this class are the leaf climbers, which seize hold of any point of support either by the foot-stalks of their leaves, or by a prolongation of the midrib. Here, also, there is the power of revolving at various rates. But though no very sharp line of demarcation can be drawn between the twiners and the leaf-climbers, and some few of the latter "can ascend by twining spirally round a support;" yet the general object of the revolving motion is here to bring the foot-stalks or the prehensile tips of the leaves into contact with surrounding objects. The leaves are sensitive to a touch and to continued pressure even when very slight. Leaf-climbing may be easily understood by observing the species of *Clematis* and *Tropœolum*, including the common nasturtium. This plant, if it meets with a string or a thin twig, casts a hitch around it with one of its leaf-stalks, and thus secures a point of support.

More highly specialised are the plants which climb by the aid of tendrils, which Mr. Darwin defines as "filamentary organs, sensitive to contact, and used exclusively for climbing." These organs "are formed by the modification of leaves with their foot-stalks, of flower-peduncles, branches, and possibly stipules. In this group, which includes the vine, the pea, and a number of Bignonias, the climbing organisation reaches its highest development. The twiner, in ascending a tree by its spiral folds, must, in order to reach the light above, describe a line very much longer than the perpendicular height to which it rises. Consequently it is compelled to expend a relatively large amount of matter. But the tendril-climber can ascend nearly in a straight line. The action of the tendrils is very curious. They revolve, and the shoot of the plant not unfrequently revolves also. If they touch any object they immediately begin to coil round it if thin enough, and become at the same time very much thicker and stronger. Tendrils which do not succeed in clasping anything generally wither and fall off. If the object found is too thick to be clasped, the points of the tendrils in some plants "exhibit a singular habit, which in an animal would be called an instinct." They continually search for any little chink or hole into which they may insert themselves. In other cases the ends of the tendrils are converted into flat discs, which are pressed close to the surface up which the plant is climbing. *Bignonia Tweedyana*, which Mr. Darwin has carefully studied, "combines four different methods of climbing generally characteristic of different plants, namely, twining, leaf-climbing, tendril-climbing, and root-climbing."

Among true root-climbers we fine a curious phenomenon—*Ficus repens*—a plant which creeps up a wall exactly like ivy, secretes from its rootlets an adhesive fluid, by which they are cemented to the wall or rock. This fluid was found to be slightly viscid, and on exposure to the air did not dry up. From experiments and observation made it would appear that the rootlets "first secrete a slightly viscid fluid, subsequently absorb the watery parts, and ultimately leave a cement." This appears to be a modified form of caoutchouc—a substance in which the genus *Ficus* is well known to abound.

A careful consideration of climbing plants can scarcely, in our opinion, fail to furnish evidence in favour of the doctrine of evolution. If we place any plant in the open ground, freely exposed to light and air from every side, we find it generally assume a compact, rounded habit; but if we set it where light and air are more or less cut off, as near lofty trees, among bushes, or in a thick wood, then two cases are possible. If the soil is poor, and if moisture is deficient, the plant will languish or even die; but if the earth be fruitful and moisture abundant, it will shoot out long, slender stems, seeking to win its way to the light. As gardeners and farmers often say, it will be "drawn" by the overtopping objects. Thus, then, the very circumstances which would render it necessary or desirable for a plant to climb, enable it, at any rate, to take the first step, by becoming more slender, longer in its joints, and more flexible.

If, as seems not unlikely, from several facts detailed in the work before us, there is a latent tendency to revolve in the shoots of all plants, this very attenuation and elongation will remove what was before a hindrance, and the power of twining may thus be gradually developed. Those forms which thus became climbers would in certain situations enjoy a great advantage over their rivals. We find further that the twining faculty appears in plants in many different grades. In some it is highly developed, in others dormant. There are cases where it appears to have been scarcely attained, and others in which it is becoming obsolete. From the twiner to the leaf and tendril-climbers the way is paved by small gradations. We cannot fail to recognise that the latter especially must have nearly the same advantage over twiners as these have in turn over plants unable to climb at all.

We cannot better conclude this brief and necessarily imperfect survey of a profoundly interesting subject than by quoting a portion of the final paragraphs of the work before us:—"When we reflect on the wide separation of these (climbing plants) in the series, and when we know that in some of the largest well-defined orders, such as the Compositæ, Rubiaceæ, Scrophulariaceæ, &c., species in only two or three genera have the power of climbing, the conclusion is forced on our minds that the capacity for revolving, on which most climbers depend, is inherent, though undeveloped in almost every plant in the vegetable kingdom.

It has often been vaguely asserted that plants are distinguished from animals by not having the power of movement. It should rather be said that plants acquire and display this power only when it is of some advantage to them; this being of comparatively rare occurrence as they are fixed to the ground, and food is brought to them by the air and rain."

A Course of Practical Instruction in Elementary Biology. By T. H. Huxley, LL.D., Sec. R.S., assisted by H. N. Martin, D.Sc. London: Macmillan and Co.

This work is arranged upon a somewhat novel plan. The author describes in succession yeast, protococcus, the proteus animalcule, bacteria, moulds, stone-worts, the bracken-fern, the bean-plant, the bell-animalcule, the fresh-water polypes, the fresh-water mussel, the fresh-water crayfish and the lobster, and, lastly, the frog. After the description follows in each case a section headed Laboratory Work, and containing instructions for the practical examination of the plant or animal in question. By way of a specimen of the task thus set the student, we insert an abridgment of the "Laboratory Work" on the *Amœba*:—"Place a drop of water containing *Amœbæ* on a slide, cover with a cover-glass, avoiding pressure, and search over with a

¼ inch obj.; having found an *Amœba*, examine with a higher power. Note—

1. *Size.*
2. *Outline.*
3. *Structure.*
    - *a.* Outer hyaline border (*ectosarc*) tolerably sharply marked off; granular layer (*endosarc*) inside this, gradually passing into a more fluid central part.
    - *b.* *Nucleus* (absent in some specimens), a roundish, more solid looking particle which does not change its form.
    - *c.* *Contractile vesicle;* in the ectosarc note a roundish clear space which disappears periodically and after a time reappears; its slow diastole — rapid systole. Not present in all specimens.
    - *d.* *Foreign bodies* (swallowed); diatom cases, *Desmidiæ*, &c.
4. *Movements.*
    - *a.* Watch the process of formation of a *pseudopodium.*
    - *b.* Locomotion.
    - *c.* If the opportunity presents itself watch the process of the ingestion of solid matters.
    - *d.* Observe movements on hot stage; as the temperature approaches 40° C. they cease.
    - *e.* Effects of electrical shocks on the movements.

5. *Mechanical analysis.* Crush. The whole collapses except the nucleus, and even that after a time disappears.

6. *Chemical analysis.* Treat with magenta and iodine. The whole stains, leaving no unstained enveloping sac. Iodine, as a rule, produces no blue colouration; if blue specks become visible it is probable that the starch they indicate has been swallowed.

7. Look for encysted specimens, and for specimens which are undergoing fission.

8. Another form of *Amœba* is sometimes found much less coarsely granular, having no well-defined ectosarc and endosarc, and having much longer, more slender, and pointed pseudopodia."

It must be admitted that a systematic course of such work will not only fix the characteristics of an animal or plant much more clearly and enduringly in the memory of the student than any amount of mere reading, but will be an admirable mental discipline. He who cannot thus learn the precious art of observation may dismiss the subject as outside his capacity.

It will be perceived that Professor Huxley regards the "study of living bodies as really one discipline, which is divided into zoology and botany simply as a matter of convenience." He holds that "the scientific zoologist should no more be ignorant of the fundamental phenomena of vegetable life than the scientific botanist of those of animal existence." This view can scarcely be disputed. There is much that is essentially common to plants and animals, and the tendency of modern research goes more and more to remove those arbitrary landmarks once set up between the two organic kingdoms. No one now considers the lowest animal as one stage above the highest plant. At the same time, we must be cautious. Whilst all that is common to the two great divisions of the organic world should be deemed equally necessary for the botanist and the zoologist, and while the methods employed by both are identical, and while neither can afford to be ignorant of the main results of the other, few persons have the time and the opportunities to enter into the details of both. The attempt to be great in many departments of knowledge leads too commonly to failure in all. The man who aims at being "good all round" in science loses his way even more signally than the "jack of all trades" who is "master of none." It need scarcely be said that the paltry jealousies which formerly existed between the respective students of animal and vegetable life have passed away. There is, we believe, only one living person—a member, if we mistake not, of the "Victoria Institute"—who has displayed his bad logic and worse taste by likening botanists to greengrocers.

The road to a sound and thorough knowledge of zoology and botany obviously lies, according to Professor Huxley, through morphology and physiology. It seems strange to us, in these days to refer to the works of Swainson—who within the memory of many still living was considered the most philosophic naturalist of the day—to find him proclaim these disciplines unnecessary, if not actually injurious to the student of animated nature. It is quite true that a man totally ignorant both of morphology and physiology may identify species, note their localities and observe their habits, and in this way may do science good service. But if we wish to study thoroughly any one animal, or class of animals, we shall find ourselves baffled and bewildered at every step if our knowledge extends no farther than its surface. Swainson considered it absurd to suppose that we could not understand an animal or plant without taking it to pieces. But neither animals nor plants exist for us to classify. Nature has not labelled and pigeon-holed them for our convenience. If we wish to understand and arrange them in any rational manner, we need every help that their internal as well as their outward structure can give us. Few persons have been foolish or eccentric enough to study Natural History from books only. Almost every zoologist, every botanist, collects specimens, and it is fortunately hard to do this without doing something more, without noting to some extent the economy and the localities of the specimens collected. But too many are utterly wanting in that solid foundation which Professor Huxley seeks to supply in the work before us. Even the determination of species is effected by many merely by *rote* without any knowledge of principles. We have been present at a botanical meeting in the north of England conducted in this spirit. A promiscuous assemblage of plants had been brought in and were laid at the head of the table. The president took up one, pronounced its generic and specific name, and handed it to his right-hand neighbour. He in turn examined it, if he thought needful, and passed it on, repeating the name, which, by the time the plant had travelled round the table was often strangely travestied. In this manner numbers of men learn by *rote* the names of all ordinary species in the British flora, but if they were asked the reason for referring any particular plant to some given genus they would be unable to reply. Precisely the same practice exists as regards insects. They learn that such and such British butterflies bear the generic name *Vanessa*, but if shown an exotic *Vanessa* they would be at a loss where to place it. Surely no branch of Natural History should be left to rest upon such a foundation.

Theory of Heat. By J. Clerk Maxwell, F.R.S., &c. Fourth Edition. London: Longmans, Green, and Co.

This book, though it may be pronounced indispensable for every student of physics, is not, in the ordinary sense of the word, a manual of the science of heat. Its object, as declared in the preface, is "to exhibit the scientific connection of the various steps by which our knowledge of the phenomena of heat has been extended." For an account of many important experiments on the effects of heat, which could not have been introduced without extending the work to an inconvenient bulk, the reader is referred to other well-known publications.

The most profound interest attaches to the last section in which the distinguished author treats on the "nature and origin of molecules." The molecules of the same substance, he declares, are all exactly alike, but different from those of other substances. There is no regular gradation in their mass from that of hydrogen up to that of bismuth, but they fall into certain classes or species. Unlike the case of plants and animals, it is not possible, Dr. Maxwell maintains, to account for the state of molecules by natural selection. Each individual is permanent; there is neither generation or decay, nor even any difference between the individuals of each kind. Hence the doctrine of evolution is here inapplicable. Hence to account for the equality of molecules in magnitude and in their natural periods of vibration we must either, as the author once expressed this view, declare that they bear the stamp of manufactured articles, or consider them as the primordial materials of the cosmos, the bricks with which its architects—conscious or unconscious, mediate or immediate—have had to work. Neither of these hypotheses is really satisfactory. To pronounce them primordial is something like the explanation a savage might give of the origin of a mountain. To declare them manufactured is merely to elicit further question as to their raw material.

Elementary Treatise on Physics, Experimental and Applied, for the Use of Colleges and Schools. Translated and Edited from GANOT'S "Eléments de Physique." By E. ATKINSON, Ph.D., F.C.S. Seventh Edition, Revised and Enlarged. London: Longmans, Green, and Co.

THE fact that this book, in its English version, has reached its seventh edition since 1863 deprives us of the ordinary scope for criticism. Ganot's work is known and appreciated by teachers and students here as well as on the Continent. The present edition contains seventeen entirely new illustrations, whilst the additions to the text amount to twenty-seven pages. An appendix has also been added containing a series of numerical problems and examples in physics, arranged so as to afford students who have not the advantage of regular professorial instruction a means of testing the accuracy and thoroughness of their knowledge. We have every reason to believe that the present edition of this book will be received even more favourably than the foregoing.

A Dictionary of Science, Literature, and Art. Edited by W. T. BRANDE, D.C.L., F.R.S. L. and E. (late of Her Majesty's Mint), and the Rev. G. W. Cox, M.A. New Edition. London: Longmans, Green, and Co.

THIS work is, in point of fact, an encyclopædia, embracing nearly the whole extent of human knowledge. Its total extent being merely three octavo volumes of some nine hundred pages each, minuteness of detail cannot be expected. The distribution of the space at command in a publication of this nature is a matter on which differences of opinion are certain to prevail. The chief attention of the surviving editor has been, perhaps naturally, turned rather to literature and art than to science. The historical, archæological, and mythological portions strike us as having been carried out on a larger scale than other subjects. Technology, chemical, physical, and mechanical, cannot be said to have fared well. Calico-printing, for instance, is despatched in nine lines, the soda-manufacture receives not quite a column, whilst "ballad" enjoys a column and a quarter, and "ballot" a column and a half. It may, however, be considered that those who take a special interest in technological questions will have recourse to Dr. Ure's "Dictionary of Arts," to which the work before us may in some sort be regarded as complementary.

The scientific articles are of very varying degrees of merit. Those treating on biological subjects are perhaps the least satisfactory. Where matters of fact are concerned, the notices are exceedingly brief, and a part of the little space at command is generally spent on the derivation of the *name* where further information on the *thing* named is urgently needed. Important genera, and even groups of higher rank, are sometimes totally overlooked. The theoretical articles, see *e.g.* "Species," are characterised by a certain feebleness and looseness of thought.

Chemistry and mineralogy are treated with greater ability, or at least with more care, whilst the articles on astronomy and mathematics will, we think, exceed both the wants and the comprehension of all readers except such as have made these sciences the subject of special study.

Having thus pointed out what from our point of view appear as imperfections, we have pleasure in adding that the work contains a vast amount of useful and valuable information. He must be indeed a learned man who in turning over these volumes does not occasionally come upon some fact of which he was before totally ignorant. This, indeed, is the chief use of encyclopædias. They are not intended to make specialists, but to supply that general knowledge without which the most accomplished specialist often finds himself "at sea," and which he has not the time nor the opportunity to seek out in its original sources.

Air and its Relations to Life. By WALTER NOEL HARTLEY, F.C.S. London: Longmans, Green, and Co.

THIS book is, as the title-page informs us, the substance of a course of lectures delivered at the Royal Institution in the summer of 1874. The character of the work is avowedly "light and popular," as its origin necessitates. Still, though scientific technicalities have been as far as possible avoided, there is no room to complain of a want of accuracy. The work is more comprehensive than might perhaps be anticipated from its title. The author treats successively of the proof of the existence of the air, of Priestley's discovery of oxygen, and Lavoisier's discovery of the nature of the air; of the reasons for regarding the air not as a true chemical compound, but as a mere mechanical mixture, and of Tessié du Motay's process for obtaining oxygen from the air. In the second chapter, Mr. Hartley treats of Black's experiments on the carbonating of lime; on the properties of carbonic acid; the presence of aqueous vapour and of ammonia in the atmosphere; the preparation of ozone; on Dr. Angus Smith's method of determining the proportion of carbonic acid in the air; and of the reciprocal action of plants and animals. In the next chapter, we find an account of the means whereby a constancy in the composition of the atmosphere is maintained. This leads to a consideration of the expansion of gases by heat, the intermixture of gases in apparent opposition to their specific gravities, and on gaseous diffusion. This naturally leads up to the subject of ventilation, with the evil effects of foul air, and a notice of Pettenkofer's recent investigations on "ground-air" and its passage into our dwellings. The remaining two chapters may be considered biological rather than physical or chemical. They treat of spontaneous generation, of minute organisms present in the air, and of the researches of Schwann, Schultze, Schrœder and Dusch, Pasteur, Pouchet, and Bastian. Concerning the conclusions of the latter author, we must call attention to the fact, that however decisively it might be proved that solutions of organic matter subject to temperatures above the boiling-point of water in sealed vessels were subsequently found to contain living organisms, not introduced from the atmosphere, such a result can throw no light on the first appearance of vegetable and animal life. On the primordial globe, before plants and animals existed, there can have been no organic matter. To prove the reality of spontaneous generation, it will, therefore, be necessary to produce low forms of organic life from purely inorganic matter, atmospheric germs being, of course, excluded by precautions similar to those adopted by Pasteur.

In connection with the important subject of ventilation and the influx of ground air into our dwellings, Mr. Hartley gives from his own observation the following interesting fact:—"A remarkable case in a London house has come to my knowledge, which gives a distinct proof of the much greater passage of gases through the walls in winter than in summer. A small room, occasionally used, was noticed sometimes to have an unbearably bad smell; this was never noticed in summer nor in winter unless a fire was lighted in the room; the drainage was suspected and examined, but was found perfect, yet here was this extraordinarily foul air making its way into the room whenever the interior was warm and the exterior cold. The cause was a dust-bin built against one of the walls, and the filtration of the air through this and the house-wall into the room." In passing, we may here remark that in all "closetted" towns, the "dust-bin" is an evil with which sanitary reformers scarcely know how to deal. It often contains matter which cannot be forced down the soil-pipe, and which is yet little less offensive than true sewage matters. It is no pleasant thing to have a heap of cabbage leaves, parings of vegetables, oyster-shells, fish-bones, &c., fermenting in close proximity to a dwelling house. Another point which naturally suggests itself in connection with the subject of "ground-air," is the danger which may follow from the adoption of asphalte roadways. These being impervious to air, the sewage gases will take the direction of least resistance, *i.e.*, they will rise up through the earth beneath the houses all the more readily on account of the ascending current of warm air within. A sound layer of asphalte should extend beneath every house, or else our dwellings, as Dr. Richardson proposes in his model city, should be built upon arches, so that the wind may have free play beneath them. Mr. Hartley very pardonably expresses a doubt as to the sanitary value of the houses built with concrete in iron frames, which may possibly not prove sufficiently porous for the walls of healthy houses. Slag he very rightly condemns as a building material, and gives from Pettenkofer an instance of a house constructed of blocks of this material being permanently and incurably damp.

The author quotes the experiments of Dr. Angus Smith, showing that carbonic acid alone, quite irrespective of organic matter derived from the lungs and the skin, &c., has a bad effect. One part of carbonic acid in 1000 of air occasioned an increase of 18 to 19 respirations per minute, the speed of the pulse at the same time being diminished. We observe the statement made on the authority of Dr. Odling, that for equal illuminating power, candles yield a larger amount of "impurity" than gas. We presume the impurity here meant is carbonic acid, but it must not be forgotten that all ordinary coal-gas contains sulphur compounds, whose products of combustion are far from desirable.

In concluding this necessarily brief examination, we must pronounce Mr. Hartley's book a valuable addition to our popular scientific literature. All persons of decent education, be they young or old, may read it with pleasure and advantage.

The Dictionary of Chemistry and the Allied Branches of other Sciences. By H. WATTS, F.R.S. Second Supplement. London: Longmans, Green, and Co.

A DICTIONARY of chemistry is in its very nature interminable. So rapid is the growth of the science that by the time the compiler has completed his task and reached the end of the alphabet, the crop of new researches which have sprung up in the meantime compel him to resume his pen and to amend, add, and rescind in accordance with the most recent authorities.

This supplement, we are told, only brings the record of chemical discovery down to the end of the year 1872, including some of the more important discoveries which have appeared in 1873 and 1874. Among the chief articles in this volume are papers on the phenols and on sulphur chlorides by Professor Armstrong; on magnetism, Prof. by G. C. Foster; on digestion, gastric juice, muscular tissue, respiration, and urine by Dr. H. N. Martin; on the chemical action of light and on spectral analysis by Professor Roscoe, and on topics belonging to agricultural chemistry by R. Warington. The articles on analysis, chemical action, gases, and many others from the pen of the editor are deserving of notice. The work may be considered as an essential requisite in the library of every chemist.

An Introductory Text-book of Zoology, for the Use of Junior Classes. By H. ALLEYNE NICHOLSON. Second Edition.

Edinburgh and London: W. Blackwood and Sons.

A WORK which, in thecompass of some two hundred pages, undertakes to give a general survey of the whole animal kingdom cannot be expected to enter into details. But this limited amount of space has been very judiciously utilised. The author, we are happy to find, devotes more attention to the "invertebrate animals than has usually been the case in introductory works on zoology." He very justly remarks that "the vertebrate animals are of no greater *zoological* value or interest than any other of the primary divisions of the animal kingdom," and adds the very important consideration that "any practical work undertaken by beginners in zoology will almost certainly lie in the department of the Invertebrata," a department, we must never forget, which embraces by far the majority of animal species, which presents the greater amount of unsolved questions, and which includes our most formidable enemies.

There are in the work certain traces of Cuvierian inspiration. Man is still placed by himself in the order "Bimana," though the author admits that the "purely anatomical distinctions between man and the other Mammals are by no means so striking as might have been anticipated." From a consideration of this passage, and of another in the introduction, it would seem that Dr. Nicholson upholds the doctrine of a marked distinction between man and the rest of the animal creation.

Another Cuvierian feature is that in a general arrangement in which man comes last, the Mollusca are placed before the Annulosa. Within the class Insecta, also, the Coleoptera follow after the "order," which includes our marvellous "six-legged rivals." In the Introduction, the stability of inorganic substances and the instability of organic matter are, in our opinion, somewhat too strongly insisted on. Dead organic matter, if preserved from the attacks of certain minute living beings, is far less destructible than might at first sight appear. The comparison of the living body to a machine is not free from dangers, and may easily mislead the student.

Still, notwithstanding these points and certain others on which issue might undoubtedly be joined, the work is one which, supplemented by judicious oral instruction, will be of great value to junior students. The fact of its appearance is, we think, a hopeful indication that the Natural Sciences are becoming more widely recognised as an essential part of a sound education, and are being taught upon sounder principles. From the abridgments of Goldsmith and the distortions of Buffon, which "*consule Planco*" were put in the heads of boys who showed a taste for Natural History, to a manual like the present is indeed a satisfactory change.

Pyrology, or Fire-Chemistry. By W. A. Ross. London: E. and F. N. Spon.

It may, perhaps, be questioned whether chemical reactions produced in the dry way, and at temperatures ranging up to full redness, have been as thoroughly studied, whether for systematic or for analytical purposes, as the changes and decompositions occurring in the wet way at temperatures not ordinarily exceeding the boiling-point of water. We have, to be sure, pyro-chemical operations on the large scale in abundance. But have all the processes of the metallurgist, limited as they are in their objects, and in the number of bodies operated upon, been found capable of scientific explanation in accordance with received theories? We have, on the other hand, in blowpipe analysis, a body of methods by which the presence of most inorganic bodies may be detected with no less ease than precision. But without at all undervaluing the results of such men as Berzelius, Plattner, Forbes, and others, we may still ask whether the standard treatises on the use of the blowpipe include every operation by which the presence of elements or of their compounds "can be discovered in the dry way?" This question Major Ross answers in the negative. Some time ago he communicated certain interesting papers to the "Chemical News," in which he made known a number of novelties which promise, at least, to be useful. Whether these new methods have been tested by mineralogists and chemists, and if so with what result, we are unable to say. We cannot lay our hands upon any memoir, English or foreign, in which they are criticised. Among these novelties, real or imaginary, are "the vesiculation of borax with oxides dissolved in it, and the corresponding crystals, which form on the surface of the vesicle, laid on cotton in ordinarily moist atmosphere; the vesiculation of boric acid containing alkaline traces, and the detection of potash in them by breathing on the vesicle; the violet colour given by cobalt oxide to phosphoric acid, and the means of thus quantitatively estimating alkalies, which turn blue in certain proportions; spherospheres, or contained balls, formed by cobalt oxide in boric acid beads; metallic-looking films formed over beads of boric and phosphoric acid held in a good hydrocarbonous pyrocone; decolouration of cobalt with soda by arsenic acid; delicate reactions of oxide of silver in phosphoric acid, by which it can be detected in most galenas; structure of pyrocones; cobalt solution, reaction given by lime; reactions of chlorine and fluorine; curious reaction of soda in pyrophosphate of lime; separation of substances, especially of metals in alloys, by utilising their different attractions for heat; the use of aluminium plate as a support; quantitative assay of sulphide of copper by oxide of lead in phosphoric acid; separation of silica, alumina, ceria, and the alkaline earths, including didymia and lanthana, by means of their behaviour in boric acid; quantitative determination of chemical water in clear fused boric acid, by means of a magnesian borate ball; separation of silica and alumina by lime borate balls; separation of didymium and lanthanum borates from ceric oxide; detection of fluorine, chlorine, and sulphur by means of oxide of copper in a phosphoric acid bead; detection of phosphoric acid in tourmaline by boric acid; new reaction obtained by a solution of manganese in sulphuric acid; mangano-cobalt solution with the same view; artificial zeolite formed by heating a mixture of potash and pure alumina, for the purpose of detecting alumina or lime, or caustic alkali; yellow and brown oxides of thallium obtained on aluminium plate from the metal; detection of sulphuric acid by the effervescence caused by adding a drop of water to a natural sulphate (as gypsum) which has been fused with soda on aluminium plate; decrepitation observed to be peculiar to crystalline forms; solution and separation of silica by boiling with boric or phosphoric acid dissolved in water; sublimation of gold, silver, and other metals by fusing them in the oxidising flame with a minute proportion of lead or charcoal over aluminium; the curious crystallisation of soda combined with a small proportion of lime; the determination of the mineral constituents of animal or vegetal organisms by burning the latter on a bead of boric acid." It is utterly out of our power to judge these novelties in the only fair manner; that is, by working through them one by one and deciding in how far the author's observations and methods can be actually verified. But this is a task which ought not to be neglected, and we are strongly of opinion that, whatever might be the result, such an undertaking would be well worth the while of any student endowed with the needful leisure, skill, and patience. Even if we suppose, for argument sake, which is extremely improbable, that the author should he found mistaken in every instance, the detection of his errors could not fail to be profoundly instructive.

We have no great respect for writers who take up some subject in an unsystematic way, and after a desultory course of reading think themselves entitled to lay down the law and to point out the supposed mistakes of received authorities. Of such men, and of their productions, every page of which testifies to the want of all fundamental intellectual discipline, every scientific critic is absolutely sick. They swarm upon us like a new plague of flies. But Major Ross belongs to a totally different category. He is no "paper-philosopher," but an earnest, careful, persevering worker, possessed of a fruitful and suggestive mind; and his conclusions, therefore, however unexpected, cannot be without value. We strongly recommend his work to the attention of chemists and mineralogists in the belief that they will find it both interesting and useful.

Modern Naval Hygiene. By Dr. LEROY DE MERICOURT, Chief of the Statistical Department of the French Navy. Translated from the French, by J. BUCKLEY, Staff-Surgeon, H.M.S. *Endymion.* London and Portsmouth: Griffin and Co.

THIS little work is, as the translator remarks in his Preface, "the first attempt to present in an English dress the mass of knowledge we have acquired" on the means of preserving the crew of a vessel in the best possible state of health. The subject, as a moment's reflection will show, is of grave interest—not merely to naval surgeons and to the authorities of Her Majesty's fleet, but to the whole nation. A sickly crew means simply an inefficient ship. The author enters upon his task by calling attention to the great difference which exists in the health of the sailor according as he is engaged,—on the one hand, in the fisheries and the coasting trade, or, on the other, in long voyages, whether in the Navy or in the Merchant Service. In the latter case he is exposed, for long consecutive periods of time, to two sources of danger—over-crowding and the effluvia from the bilges. At one time there was a third, and yet more pressing evil—sea-scurvy, arising chiefly from the difficulty of obtaining a due supply of fresh provisions. This difficulty having been mainly overcome, there remains the great question of ventilation—the supplying, constantly and regularly, pure air in every part of the ship, and the removal of all offensive emanations and of their sources. This question, not always successfully solved on land, is far more difficult at sea. It is obvious that the ventilating arrangements which might prove perfectly adequate during a cruise in the Channel may be found very deficient on a voyage down the Red Sea. The matter has been further complicated by the recent changes in naval architecture, and especially by the introduction of steam-power in men-of-war. The low free-board, the smaller number of the ports, and their lower position, are not in favour of ventilation. The space formerly allotted to the men is curtailed by the engines and coal-bunkers, over-crowding is increased, and the atmosphere of the depths of the ship is more vitiated. On the other hand, the increased rapidity of the voyages and the more frequent calling in harbour counteract to some extent the evils of over-crowding. In the most modern ironclads the number of men is reduced, and the sleeping space is nearly doubled. But we have to take into account not merely the number of cubic feet of air per man, but the ease and speed with which it can be changed in all weathers. In screw-steamers the temperature is often excessive. "In the gun-boat *Eclair,* during the summer of 1855, at Algiers, when the thermometer in the shade stood at 95°, 158° and even 167° F. were registered in the stoke-hole!" Such a temperature is not only directly injurious to the men, but greatly promotes decomposition in all organic bodies. "The process of blowing out the boilers floods the bilges with hot water loaded with saline and greasy matter." One of the evils of the high temperature is the thirst of the men, and the immoderate quantity of water—plain or acidulated with vinegar or lime-juice—consumed. "In Europe it amounts to 3 or 4 quarts, and is at least doubled in the tropics." We have observed in various chemical works that the men employed in very hot situations quench their thirst with water to which a trace of sulphuric acid has been added: according to our observation and personal experience this is a far safer beverage than any dilution of the vegetable acids, especially vinegar.

A section of this work is devoted to an exposure of the evils arising on board French ships from the use of cooking utensils lined with an alloy of tin and lead. As a matter of course lead-poisoning manifests itself in all its insidious forms. French cooks, by sea and land, appear to have an unfortunate predilection for this deadly alloy, which they call *la claire*. No metal ought to touch human food save iron or silver. The former has been too often rejected from the unfortunate mistake of taking colour into account—an idea which belongs to the dye-house, and should be totally unknown in the kitchen.

In conclusion, we feel bound to express our opinion of the high value of this book, and we hope that it will meet with a wide circulation among all persons interested in nautical matters.

Rambles in Search of Shells, Land and Fresh-water. By J. E. Harting, F.L.S., F.Z.S. London: J. Van Voorst.

It may be asked why our land and fresh-water shells have hitherto enjoyed so little popular attention? They are by no means deficient in interest and beauty; they are more easily captured than insects, and their preservation is easy. The subject, as the author remarks, may easily be studied in connection with botany, or, we may add, with entomology. Why, then, should land shells be so much less regarded than their oceanic kinsmen whose habits can be less easily studied? The entire number of species of terrestrial and fresh-water Mollusca found in the British Islands does not, as far as at present known, exceed 120. By the way, whilst we think too great attention cannot be paid to recording the geographical distribution of every form of organic life, we have never been able to understand the mania for "British specimens" common among a certain class of naturalists, or, we might rather say, of collectors. England is no natural zoological province, but merely a portion of Western Europe which has become detached from the mainland by the gradual action of the sea. The naturalist who excludes all French or Belgian specimens from his cabinet should—by a parity of reasoning—refuse to admit all that have been captured in Ireland or the Channel Islands.

We are glad to find that the author speaks somewhat contemptuously of the conchology of the past. We have sometimes, in quiet nooks where ancient ideas still linger, met with extensive collections of shells, duly labelled and arranged, and carefully preserved from dust and damage. But the collector did not in the least trouble himself with the nature, structure, and functions of the creatures who somewhile tenanted his shining specimens. To understand them was no part of his object. Such a pursuit is not a Science, but merely a "fancy," and may be placed upon the same level as collecting postage-stamps. But, as Mr. Harting well remarks, "the conchologist has given place to the malacologist, who, not content with examining, describing, and naming the shell independently of its inhabitant, curiously questions the latter as to its internal structure."

The author's object has been to give clear and correct information concerning our indigenous land and fresh-water shells, without being alarmingly technical or systematic. He gives a succinct account of the internal structure of the Mollusca, and notices their peculiarities of respiration, locomotion, and reproduction. Their classification is briefly treated, and the reader is then asked to accompany the author, in spirit, in shell-hunting rambles "over the London clay, over the chalk down, and through the moist beech-woods of Sussex,"—a method of imparting instruction which may perhaps be pronounced unsystematic, but is certainly more attractive than a description of genera and species in their due order. The illustrations have been "carefully drawn and coloured from recent specimens," and not, according to a too common practice, copied from other books. We think this little manual well calculated to attract votaries to this somewhat neglected branch of natural history.

The Dawn of Life; being the History of the Oldest-Known Fossil Remains. By J. W. Dawson, F.R.S., &c. London: Hodder and Stoughton.

We have here a monograph of the *Eozoon Canadense*, the most ancient relic of the animal world hitherto discovered, or, as the author more magniloquently expresses it, "the earliest known representative on our planet of those wondrous powers of animal life which culminate and unite themselves with the spirit-world in man himself." The subject is of profound interest to the geologist and biologist, involving as it does "the opening of a new era in geological science," and pushing the first origin of animal life backwards in time for untold ages. But that it throws any light on the question of Evolution, favourable or unfavourable, we do not see.

The author proposes "to give a popular, yet as far as possible accurate, account of all that is known of the dawn-animal of the Laurentian rocks of Canada," including a description of the formation itself; "a history of the steps which led to the discovery and proper interpretation of this ancient fossil; the description of *Eozoon*, and the explanation of the manner in which its remains have been preserved; inquiries as to forms of animal life, its contemporaries and immediate successors, or allied to it by zoological affinity; the objections which have been urged against its organic nature, and the summing up of the lessons in science which it is fitted to teach." The latter section of the work is undoubtedly its feeblest part, the earlier chapters being a clear record of facts.

The Laurentian rocks were first recognised as a special geological formation in Canada, where they are strongly developed in a range of hills to the north of the St. Lawrence valley, and named the Laurentides by the old French geographers. They are "the deepest and oldest of all the formations known to the geologist, and more thoroughly altered or metamorphosed by heat and heated moisture than any others." They formerly received the name of Azoic, being, as was then considered, utterly destitute of all traces of animal or vegetable life, but are now designated Eozoic, as "those in which the first bright streaks of the dawn of life made their appearance." The same formation appears in the Adirondack mountains of the State of New York, and in various patches along the American coast from Newfoundland to Maryland. "The older gneisses of Norway, Sweden, and the Hebrides, of Bavaria and Bohemia, belong to the same age, and it is not unlikely that similar rocks in many other parts of the old continent will be found to be of as great antiquity."

The recognition of the Laurentian formation was not by any means at once followed by the discovery of its characteristic fossil, the *Eozoon*. The extensive alteration which the rocks had undergone rendered such a discovery little probable. Lyell, Dana, and Sterry Hunt, however, inferred that there were certain sound reasons for believing that organic remains might be detected even here. They argued substantially thus:—If the Laurentian rocks are altered sediments it follows, from their vast extent, that they are an oceanic deposit. But had there been no living thing in the water, they would merely have consisted of the sandy and muddy *débris* abraded from igneous rocks by the action of the sea. But the Laurentian formation contains beds of limestone above a thousand feet in thickness, and extending for hundreds of miles. Now, limestone is an organic formation. "When," as the author remarks, "we find great and conformable beds of limestone, such as those described by Sir W. Logan in the Laurentian of Canada, we naturally imagine a quiet sea-bottom in which multitudes of animals of humble organisation were accumulating limestone in their hard parts, and depositing this in gradually increasing thickness from age to age. Graphite is another important constituent of the Laurentian rocks, occurring not in veins or fissures, but in regular layers in the substance of the limestone or gneiss, and forming, according to the author's calculation, in one division of the Lower Laurentian of the Ottawa district, an aggregate thickness of 20 to 30 feet. But vegetable life is the only known agency capable of withdrawing carbon from the carbonic acid of the atmosphere, and depositing it as a constituent of rocks. Hence the existence of plants in the Laurentian age, as well as of animals, becomes highly probable. The Laurentian formation, further, exhibits beds of iron oxide, sometimes 70 feet in thickness—another evidence of organic action.

The actual discovery of organic remains was made by Sir W. Logan, and announced at the Springfield meeting of the American Association for the Advancement of Science in 1859. The organic nature of the *Eozoon* was not, however, at once admitted. Certain men of science maintained that the new-found fossil was of inorganic nature. A discussion ensued, in which Dr. Hunt, Dr. Carpenter, and others took part. Slices of the specimens, in comparison with similar sections of every variety of Laurentian, primordial, and Silurian limestones, and of serpentine marbles, were microscopically examined with ordinary and polarised light. Dr. Hunt undertook a chemical investigation of the associated minerals, the final conclusion being that the structure was organic and foraminiferal, and that it could be distinguished from any merely mineral or crystalline forms occurring in these or other limestones.

The fossil, then, is "the skeleton of a creature belonging to that simple and humbly organised group of animals which are known by the name of Protozoa." It has kindred still existing, and it is even possible that a living specimen of the *Eozoon Canadense* itself may be dredged up from the waters of the Atlantic or Pacific. Foraminiferal animals have, as a whole, been diminishing in size in the lapse of geological time. On this subject the author remarks—"It is, indeed, a fact of so frequent occurrence that it may almost be regarded as a law of the introduction of new forms of life, that they assume in their early history gigantic dimensions, and are afterwards continued by less magnificent species."

The last chapter of the work—"The Dawn-Animal as a Teacher in Science"—is of a much more speculative and less trustworthy character than what precedes. The *Eozoon's* teachings are too evidently "inspired" by Dr. Dawson. The author places great confidence in the silence of the "stone-book." "We have," he says, "for example, no connecting-link between

Eozoon and any form of vegetable life." Be it so; the fact proves absolutely nothing. The further back we go in our researches the greater number of forms—both of vegetable and animal life—must have disappeared, and left not a trace behind. Why should the connecting-links have been especially preserved? And if they had, would not those upon whom the mantle of Agassiz has fallen, and who talk of "prophetic analogues," refuse to recognise them? What can we think of the following passage?—"There may perhaps be higher intelligences that find it equally difficult to realise how life and reason can manifest themselves in such poor houses of clay as those we inhabit? Dr. Dawson cannot imagine that "clay" is a constituent of the human system. Why, then, does he use language which may mislead men of defective education, and which is certainly more worthy of a penny tract or of a "goodie" story-book than of a scientific treatise? Had he concluded his book with the seventh chapter he would have done a better service to science and to the public.

Fifty-fifth Annual Report of the Board of Public Education of the First School District of Pennsylvania, comprising the City of Philadelphia, for the Year ending December 31st, 1873. Philadelphia, 1874.

THIS volume gives a detailed account of the public schools of all grades in the city of Philadelphia, including the numbers of the pupils, the names of the teachers, the subjects taught, &c. We notice as a curious fact that in schools for boys the teachers, as a rule, appear to be females. What has been the motive which led to so singular an arrangement, or what are the benefits—real or supposed—resulting, we are not informed.

The President's Report is an ably composed document, and must be interesting on this side the Atlantic as showing the projects and the hopes of many thoughtful men in America. We are struck, in particular, with this passage:—"Let me ask you, gentlemen, does not justice demand that we should place within the reach of girls the same unrestricted privileges for pursuing the higher branches of learning that we extend to boys?" It might as well be asked—"Does not justice demand that we should place within the reach of girls unrestricted facilities for acquiring a knowledge of military drill, seamanship, or the use of heavy artillery? We fail to see that science or the human race are likely to be benefitted by enticing or thrusting normal women into pursuits which—not by any arbitrary convention, but by a process of natural selection—have been hitherto reserved for the male sex. We fear that Institutions where girls may "seek graduating honours" will merely tend to develop a class of epicenes possessing the defects of both sexes and the merits of neither. We submit that mannish women should be regarded with the same abhorrence which we instinctively mete out to womanish men.

The writer, in a subsequent passage of his report, discusses the question of compulsory education, which in America, as in England, appears to be attracting great attention. It appears that in the Philadelphia district, although 100,000 pupils attend the public schools, yet no fewer than 20,000 children are stated to be "growing up in ignorance and vice." This is a much larger proportion than we should have expected. "To give pauperism its death-blow we must rescue the children now beyond the pale of our public schools." But will education, even if universal, really extirpate pauperism? We fear not. We must remember that, like most other possessions, the value of a good education is governed by the laws of supply and demand. The more general it becomes, the less is its value to the individual. Where educated men are few they can name their own terms, and, except dishonest or dissipated, may make sure of a comfortable position. But where there are more educated men than spheres of activity requiring their services, some must certainly go to the wall. Few persons in modern England are in a more deplorable position than such as have received a good education, but have no special knowledge of any profession, business, or manufacture. Compulsory education, by increasing the number of this class, will merely make the competition among them more frantic. It is—at least as far as England is concerned—an error to suppose "illiteracy" the only, or even the main, cause of pauperism. We see men scarcely able to sign their own names, or to speak their native language with moderate accuracy, amassing millions. We see, on the other hand, men of wide and profound culture, and even of original thought, earning a bare pittance. We can scarcely point out an instance where a discoverer, an inventor, a man who has enlarged the boundaries of human knowledge, has accumulated a fortune by his labours.

The pauper, again, often inherits a low vital tone: no school can cure that. He frequently inherits an intense craving for alcoholic excitement. Can this be overcome by any amount of literary culture?

Statistics, the writer holds, "prove by an inexorable logic that ignorance is the most prolific source of crime." They prove certainly that criminals are generally ignorant; but these two propositions, if carefully examined, will be found not identical. The man of the criminal type is ignorant because he has no innate love for knowledge; but if you force knowledge upon him it may happen that you merely make him a more formidable enemy to society. The fallacy in question is something like that into which George III. fell in his inquiry into the cause of longevity: he found that all the very old men who came under his observation agreed in one point, and in one only—they were all given to early rising. Hence the king inferred that early rising was the cause of their longevity, and that any one by persistently rising early might prolong his days; but the fact is that only men of exceptionally vigorous constitutions can regularly, and of their own free will, rise early. Such men attain a great age in virtue of their unusual vigour, of which the tendency to early rising is not the cause, but merely a sign. A feeble person, by persistently rising early, is more likely to shorten than to prolong his days.

To return: it must not be thought that we are in the least favourable to popular ignorance; but we cannot share all the dreams in which some of the more sanguine friends of education indulge. Universal literary culture will suppress pauperism and crime in the very same year when free trade effects the final abolition of war. Till then both "world-betterers" and lookers-on must possess their souls in patience.

Reports on the Physical, Descriptive, and Economic Geology of British Guiana. By C. B. Brown, F,G.S., and J. G. Sawkins, F.G.S. London: Printed for Her Majesty's Stationery Office, and sold by Longmans, Green, and Co., and E. Stanford.

A full and accurate knowledge of the resources and capabilities of every part of the British Empire must be of the utmost value to our statesmen, our Government officials (whether military, naval, or civil), and not less to our merchants and manufacturers. We are therefore glad that such tasks as the geological survey of the rich and beautiful province of British Guiana have been undertaken. We may indeed wish that these investigations were pushed forward with greater zeal, and that the expeditions sent out had the opportunity to enter more minutely into the mineralogy and palæontology of the region. Perhaps in time our wishes may be gratified. The mineral wealth of the country does not seem great. There is an inexhaustible supply of a pure white sand, especially in Demerara, admirably adapted for the glass manufacture. Clays are found of various qualities,—some suitable for hydraulic cements, and others for porcelain. Sandstones suitable for paving and building abound in the interior of the colony, but the expense of transit to the coast is at present too great to admit of its being utilised. Iron and manganese are met with in various districts. Gold has been very extensively sought for, and actually found, though not in remunerative quantities. The workings of the British Guiana Gold Mining Company, up the Cuyuni River, have been abandoned. The greatest wealth of the colony consists in its rich agricultural soils, some of which contain as much as 0·846 of nitrogen, whilst phosphoric acid reaches 0·185, and potash 0·345 per cent. The authors give a catalogue of rocks and minerals collected for the Geological Museum, in Jermyn Street. That institution, we may here remark, will have to undergo a great transformation if ever it is to represent the economic geology of the British Empire. The amount of space at its disposal is ridiculously inadequate.

The authors do not appear to have been well supplied with resources for a thorough examination of the country. Repeatedly we find in their reports passages similar to the following:— "We regret that our means of supporting labourers with the necessaries of life prevented us from carrying our examinations to the extent we would recommend." We must confess that we like to see scientific work of any kind done thoroughly. To send out qualified men, and to support them so ill that they are compelled to leave their task half accomplished and to hurry over matters which require careful investigation, is a very mistaken economy.

The surveys of the territories undertaken by the Federal Government of the United States include an account of the vegetable and animal productions of every district, as well as of its rocks, minerals, and soils. Nothing of the kind has been undertaken in the survey of British Guiana, a land literally teeming with interesting productions. For this omission we are far from blaming Messrs. Brown and Sawkins. They have evidently made good use of their time and limited facilities, and deserve great credit for the manner in which they have fulfilled their task. But we regret that a more numerous and better appointed expedition was not sent, whether by the Imperial Government or the Provincial Legislature.

The work is illustrated with a large and valuable map of British Guiana, and with several sections and diagrams.

The Annual Address of the Victoria Institute or Philosophical Society of Great Britain, June 7, 1875. By Rev. R. MAIN, F.R.S., "Radcliffe Observer." To which is added the Report for the Year. London: R. Hardwicke.

A COMPLETE review of this "address" may be pronounced difficult or rather impossible in any journal which does not profess to deal with theological questions. We must, therefore, confine ourselves to the notice of certain incidental passages.

As a matter of course, we find a reference to the works of Mr. Darwin, which are brought forward in illustration of the assumed tendency of the present day to accept "a clever hypothesis, supported on some exhibition of facts," without the necessary thorough-going scrutiny. Mr. Main, it seems, rejects the doctrine of Evolution because Professor Nicholson's conclusions, which *seem* to have been formed from a very careful consideration of the subject in *some* of its branches, *seem* to show that Darwin's theories are of very limited application." The words we have italicised throw some light on the logical rigour which Mr. Main would substitute for the rash theories and the love for hasty and paradoxical generalisation of which he accuses the nineteenth century. "The student of natural philosophy," he continues, "is, in my opinion, quite justified, on philosophical grounds, in declining to accept the ancestry offered him." Now, unless Mr. Main is a biologist, his opinion on such a question is not worth recording. He is exactly like a lawyer who should pronounce an opinion on the bearing of certain documents without understanding the very language in which they were drawn up!

After an excuse for not entering further into the Darwinian controversy, the author gives a brief survey of "the most inportant physical discoveries, chiefly astronomical, which have been made during the last few years, being careful to avoid details, and to consider them *only with reference to their bearing on religion*." In this survey it is interesting to note that he admits substantially the nebular theory of Laplace, which but a few years ago was proclaimed essentially atheistic in its tendencies, and was denounced accordingly with no less acrimony than is now bestowed upon the doctrine of Evolution, of which, indeed, it forms an integral portion. Can Mr. Main and his friends learn no lesson from all this?

The author then makes "a passing allusion to two books recently published, which exhibit perhaps the lowest stage of religious belief which has been given in this century as the result of the final and sober conclusions of two very deep thinkers, devoted the one to the study of philosophy and the other to that of biblical criticism." The works in question, Mills's "Essays on Religion" and Strauss's "Old and New Faith," do not come within our cognisance, and we must therefore pass over the examination which they receive from Mr. Main. The remainder of the pamphlet is taken up with a subject which is by this time almost thread-bare—the "Belfast Address." We have already examined this address and have declared that, in our humble opinion, Professor Tyndall went beyond the legitimate sphere of science and made a too great use of his imagination. There is consequently the less need that we should revive the discussion. One passage, however, must claim a brief notice. Says Mr. Main—"Why is Giordano Bruno set so prominently before us but because he revived the doctrine of atoms, though in a very confused way, and asserted pantheistic principles; and because he was a martyr of science, and thus a rare (?) opportunity was given of showing the cruelty and obstructiveness of the Church?" Does the author mean to deny the "obstructiveness of the

Church" or to doubt that from the days of Pericles down to the present year Ecclesiasticism has persecuted science? If so, he is a man with whom no discussion can be held. Discoverers are not now, indeed, rewarded with the dungeon or the stake; but let any one merely read over the denunciations of the Royal Society, the British Association, and the late Society for the Diffusion of Useful Knowledge uttered by ecclesiastics from the time of Charles II. to our own times, and he cannot fail to see that though the power to make "martyrs to science" is wanting, the desire has not slackened. We can point to many attacks upon science made, ostensibly at least, in the interests of religion, and far more unwarrantable than anything in Dr. Tyndall's speech. Nay, it is far from improbable that this speech was merely an injudicious counter-raid. We would refer here to the custom of preaching special sermons to, about, or perhaps rather *at*, the British Association, a custom which we think has been adopted in every town where that body has held a meeting. Too many of these sermons intimate that science is a very dangerous and questionable thing and requires very closely watching. Now, being desirous to prevent these unnecessary and unseemly conflicts between religion and science, we would suggest that all Tyndall-discourses on the one hand, and all these sermons on the other, shall be dropped. Let the clergy of any town where the British Association may meet preach as they would at any other time of the year. Let all scientific theories be suffered to stand or fall on their own evidence, without being considered amenable to any theological tribunal, and let religious dogmas receive a corresponding immunity from scientific jurisdiction. Or, putting the matter in another light, let theologians and anti-theologians, if fight they must, no longer select the territories of science for their battle-field. We fear, however, that such a proposal would be very unfavourably received by Mr. Main and his friends of the Victoria Institute.

Annual Report of the Board of Regents of the Smithsonian Institution. Washington: Government Printing Office. 1874.

THIS volume is particularly valuable as containing elaborate biographical notices of two illustrious men of science not long ago deceased—Charles Babbage and Louis Agassiz. The former, we fear, in his own country at least, scarcely occupies that high place in public estimation to which he is so fully entitled. His enmity to street music is a standing joke among those who never think intensely, and who therefore cannot comprehend the distracting effect of such noise upon a busy brain. But we almost question whether in this matter Babbage did not "strain at a gnat and swallow a camel." Bad as may be the grinding-organ and the German band, they must yield the palm of nuisance to the monotonous throb of the "mission-room" bell and the solemn howls of the harmonium in the house of a musical neighbour.

Minds of a somewhat higher order remember the name of Babbage in connection with an unfinished calculating machine which, in due obedience to the modern gospel of success, they class with the attempts of visionaries to square the circle or find the philosopher's stone. Now, that Babbage was not a successful man we admit. It is perfectly true, quoting from the work before us, that "there was not a place which he ever sought that he gained. He aspired to the Professorship of Mathematics at the East India College at Harleyburgh, to Playfair's chair at Edinburgh, to a seat at the Board of Longitude, to the Mastership of the Mint, and to the office of Registrar-General of Births and Deaths, and failed in all." His *magnum opus*, the great analytical engine, never was completed, But in the opinion of the numerous and eminent mathematicians and engineers who had examined the inventor's plans, success, in the fullest sense of the word, was merely a question of the needful funds. The machine was intended to contain a hundred variables, each consisting of 25 figures; it would calculate a thousand values (of *e.g.*, a, b, c, d by the formula—

$$p= \frac{\sqrt{a+b}}{c\quad a}\Big)$$

print them, and reduce them to zero. "When the machine wanted a tabular number it would ring a bell and then stop itself. On this the attendant would look at a certain part of the machine and find that it wanted the logarithm of a given number, say of 2303. He would then go to the drawer, take the required logarithm card, and place it on the machine. Upon this, the engine would first ascertain whether the assistant had or had not given it the correct logarithm of the number: if so, it would use it, and continue its work. But if the engine found that the attendant had given it a wrong logarithm, it would ring a louder bell, and stop itself. On the attendant again examining the engine, he would observe the words WRONG TABULAR NUMBER, and then discover that he had really given the wrong logarithm, and, of course, would have to replace it by the right one."

That such a conception should remain incomplete even after its possibility was demonstrated, is a disgrace to the age. "There was not an invention connected with his name, and in mathematical mechanics he ranks among the foremost the world ever produced, which, in the opinion of the best disciplined minds of his day, he could not have perfected had sufficient pecuniary means been at his command.

Among his literary productions we find honourable mention made of the "Ninth Bridgewater Treatise." His no less remarkable work, the "Decline of Science in England," has escaped notice. We fear that not a few recent circumstances could be found to support the position of the author, and that the tide which he regretfully pointed out has not yet turned.

The career of Agassiz is one which we must contemplate with a strange mingling of satisfaction and regret. We rejoice that he effected such great things for science; we sympathise with his enthusiasm in the cause of discovery; we fully appreciate the fearlessness with which he attacked many of the most rooted prejudices, in connection, for instance, with the notion of a broad and abrupt boundary line between man and the rest of the animal creation; but we view with astonishment and grief his attitude in connection with the great doctrine of evolution. He received the first announcement of a theory which has shed such an invaluable light upon organic science, not like a philosopher, critical, or if you will, even sceptical, but like an eager partisan who feels his interests at stake. The explanation of this painful fact may be deduced from the work before us. His brain, like that of many a noble follower of science, was giving way under the pressure of severe and continuous study. Had he lived longer he would have outlived his real self. There can be no doubt that at the time when the views of Darwin were first brought under his notice he was already labouring under incipient cerebral disease. We do not desire to dwell further upon the spectacle of the decay of a genius so exalted. Agassiz was a man for whom the world should feel thankful, and with whose final short-comings it should deal reverently.

Magnetism and Electricity. By FREDERICK GUTHRIE, Professor of Physics at the Royal School of Mines. With 300 illustrations. London and Glasgow: William Collins, Son, and Co. 1876.

WORKS on electricity have somewhat multiplied of late, but with one or two exceptions they have belonged to the popular and slight class of books, rather than to the solid and precise student's manual. For many years De la Rive's "Electricity" was the standard work on the subject, supplemented, of course, by Faraday's admirable "Electrical Researches;" and, from the more popular point of view, books like Noad's "Manual." Now we have Clerk Maxwell's magnificent mathematical treatment of electrical phenomena, and the numerous papers of Sir William Thomson, which form a treatise in themselves, and one or two works like Fleeming Jenkin's "Electricity," in which many of the most recent mathematical deductions are introduced, and are treated in a more or less popular manner. Dr. Guthrie's work belongs to this last class. It is based upon the lectures which he has been in the habit of giving, during the last six years, at the School of Mines to mining students and science teachers, and he now brings it forward in an extended form in the hope that the work may be of service to a larger class of the community.

The first book, consisting of ten chapters, treats of Frictional Electricity, and commences with an account of attraction and repulsion, and the dependence of the former on induction. Under the head of Electrical Machines, the new machines of Holtz and Bertscl are described and figured, and the clearest account of the former we have ever seen is given here. Condensers and the effect of the discharge follow, a short chapter on Dielectrics, a chapter on various sources of electricity besides friction, and, finally, a chapter on Electrical Measurement.

In this we find some extremely useful definitions, thus as to *unit of electricity*, the author observes :—" Each of two equally charged bodies is said to have a unit of electricity if, when at a distance of one centimetre from one another, the one will repel the other with a force which in one second of time would impart a velocity of one centimetre a second to one grain of matter." Again, as to that difficult definition the Electrical Potential:— " The work which a raised body is capable of doing, if it fell a certain distance, is called the *potential* work of this body, or simply its (mechanical) potential. . . . . Instead of a unit of weight, let us take a unit of electricity. . . . . The electrical potential of a body is the mechanical work which the electricity of the body is capable of doing in passing to the earth, or other indefinitely great reservoirs of electricity of the same kind as the earth's. . . . . Potential may be defined as the preparedness to do work." The chapter concludes with an account of Thomson's Electrometer.

The second book treats of Voltaic Electricity, and consists of twelve chapters, among which may be specially noted those on Measurement, on Resistance Conductivity, Electro-Motive Force and their Measurement, and on the Relation of Electricity to Life. In the latter, we are told, in insectivorous and sensitive plants a current is established at the moment when the leaf is irritated, and when consequently it moves. Again, although there seems to be a somewhat intimate connection between nervous and electrical excitement, the strongest coil-current applied to the head as a cap does not appear to affect the thinking faculties. Neither does the act of thinking appear to produce a current. The third and final book relates to Magnetism, and this is followed by two appendices containing useful experimental hints.

Dr. Guthrie's book, without being a popular treatise in the broadest sense of the term, combines facile expression, and an absence of abstruse treatment, with an account of the more recent mathematical development of the science. It is essentially a student's manual: thorough and precise, without being overloaded with dry details and technicalities. The experimental details are valuable, as we may be sure that all the experiments described have been repeated at South Kensington, and not merely introduced on the hearsay of others. Altogether the book is to be welcomed as a real addition to our scientific literature.

Catalogue of the Publications of the United States Geological Survey of the Territories. By F. W. Hayden. Washington: Government Printing Office.

"The geologist in charge is desirous of securing, by exchange, the publications of foreign countries on geology, palæontology, and natural history generally, to aid in the formation of a library of reference for the use of the survey of which he has charge.

"He avails himself of this opportunity to again ask those persons or societies that may receive the publications of the survey to reciprocate by sending to him such of their own publications as they may feel disposed, and he believes that he can assure them an ample return, either in books, or specimens, or both.

"The reports of surveys with maps, charts, and sections, transactions of societies, or the publications of individuals engaged in scientific studies, are much desired as works of reference.

"Parties who may look favourably upon the above proposition can send all packages, through the Smithsonian Institution, to the address of Dr. F. W. Hayden, U.S. Geologist, Washington, D.C.

"Societies, libraries, or persons engaged in active scientific investigation, desirous to receive the publications of the survey will confer a favour by communicating their wishes."

Whilst we feel great pleasure in giving publicity to this appeal, we feel it our duty to call attention to the truly magnificent character of the survey now being executed on behalf of the Government of the United States. Not merely the geology, but the mineralogy, botany, zoology, meteorology, ethnology, and antiquities of the whole union are being carefully explored. Will the day never come when a similar survey will be made of the still wider and more varied regions included within the British empire? It would be a glorious contribution to science and a worthy bequest to posterity.

CORRESPONDENCE.

AËRIAL LOCOMOTION.

SIR,—In your issue of October, 1875, you publish a letter from Professor Marey in reply to an article by Professor Coughtrie entitled "Pettigrew *versus* Marey"[*] which requires a few words of comment on my part. In the article in question Professor Coughtrie, as your readers will remember, prefers what many of them will regard a charge of plagiarism against Professor Marey, and quotes in support a series of parallel passages and dates which one would naturally have expected Professor Marey would either have explained or refuted, but which singularly enough he has not done.

As Professor Marey in his reply makes it appear that I have intentionally or otherwise given a mutilated version of the letter addressed by him to the French Academy, in which he assigns me priority in the discovery of the figure-of-8 and wave movements made by the wing in space, you would do me a favour by publishing that letter in full. In order that the reader may have all the facts before him I will, with your permission, refer to the entire correspondence between the French Academy, Professor Marey, and myself.

(I.) *Letter of Reclamation addressed by Dr. Pettigrew to the French Academy of Sciences.*

"Royal College of Surgeons of Edinburgh,
"March 28, 1870.

"The Perpetual Secretaries of the French Academy.

"Gentlemen,—Having had my attention directed to two papers communicated by Professor Marey to the Academy of Sciences on the 28th of December, 1868, and the 15th of March, 1869,[†] in which he describes and puts forth as a new discovery the peculiar figure-of-8 movements made by the wing of the insect during its action, it has been suggested to me that in justice to myself I ought to inform the Academy that the figure-of-8 theory of wing movements was first promulgated by me in a lecture delivered at the Royal Institution of Great Britain in March, 1867. An abstract of the lecture was translated into French and appeared in the "Revue des Cours Scientifiques" of 21st September, 1867, along with two other papers—the one by Professor Marey, the other by M. Armand Angliviel. * * * [Here follow references., &c.] I have taken the liberty of submitting a copy of my memoir to the Academy in the hope that it may graciously consider my claim to be regarded as the original discoverer of the *figure-of-8 movements* made by the wings of insects, bats, and birds, when artificially fixed, and of the *spiral and undulatory wave tracks* described by the wings of insects, bats, and birds, when the said insects, bats, and birds are flying at a high horizontal speed.

"I have only to add that my lecture appeared in the "Proceedings of the Royal Institution of Great Britain" under date the 22nd of March, 1867, nearly two years before Professor Marey's first communication to the "Comptes Rendus;" my memoir of which the lecture formed a part having been read to the Linnean Society in less than three months after the lecture was published, viz., on the 6th of June, 1867.—I have the honour to remain, &c."

(II.) *Letter Addressed by Professor Marey to Dr. Pettigrew before Replying to the Letter of Reclamation Lodged by Dr. Pettigrew with the French Academy.*

"Paris, April 29, 1870.

"Sir and Honoured Colleague,—I

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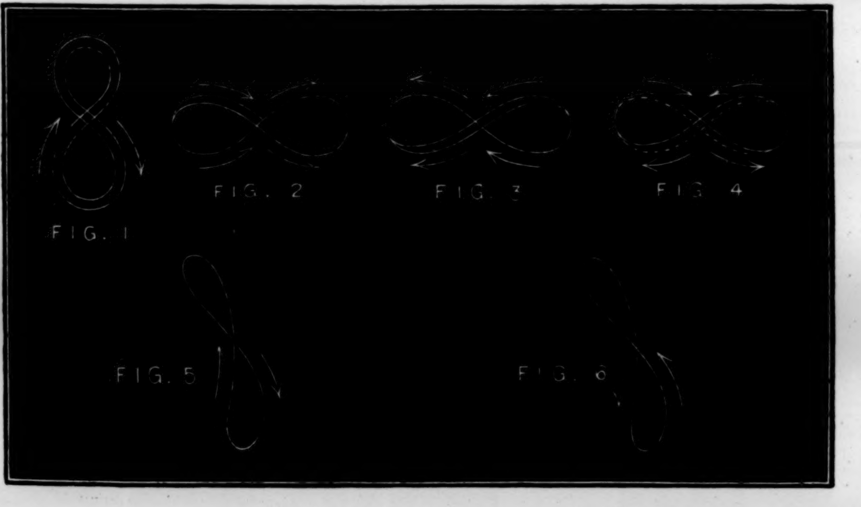
[*] QUARTERLY JOURNAL OF SCIENCE for April, 1875.

[†] These communications are printed in the "Comptes Rendus" under the dates specified.

was absent from Paris when your reclamation was produced on the subject of the figure described in space by the wing of insects and birds; it is on that account that I have delayed to answer your note. It appeared to me that it was best to address myself to you directly. Indeed, I believe there exists between us a pretty considerable disagreement as to the manner in which the figure-of-8 described by the insect is generated. I do not hesitate to acknowledge that in your memoir you have pointed out certainly before me the figure-of-8 presented by the wing of the insect during flight, and I regret not having had earlier knowledge of your work in order to assign to you that priority. One of my audience, however, told me last year that Lacordaire had formerly pointed out that figure of movement of the wing; but I have not succeeded in finding that indication in his works on entomology. In regard to the interpretation of that appearance of the wing in motion, here is, I believe, the point in which we disagree. On referring to page 233 of your memoir, diagram 5, if I have rightly understood your exposition, the thick line indicates the passage of the thick margin of the wing, the narrow line the passage of its thin border; these two lines are generated in one single movement of the wing, which in the case of diagram 5 is carried from left to right, as the direction of the arrows indicate. In the copy of my work which I have the honour to address to you you will see at page 171, figure 16, that I find that the two branches of the 8 are engendered by two movements alternating and in opposite directions produced by the wing of the animal, as is shown by the reverse direction of the two arrows. In my experiments I applied gold leaf to a point of the extremity of the wing, and it is the track made by this single point which describes the contour of the 8. In your diagram you ascribe to the two opposite borders the formation of the figure-of-8, so that (diagram 5) the thin border which is formed above at the beginning of the movement passes below at the end of the movement. For me the narrow border does not pass thus from one side to the other of the thick border, and when we place a little speck of gold on each of these two borders we see formed two figures of 8 parallel with each other, as I represent it here. [Fig. 1 of present communication.] I consider the changes of plane and all the deviations of the course of the wing as the effect of the resistance of the air, in which the wing is entirely passive. If my imperfecct knowledge of the English language has not led me into error, the wing, according to you, would change its form actively, and would rotate on its articulation as if to screw itself in the air. Experiments which I have made by



means of a mechanical insect have shown me that the mechanism of flight may be obtained quite well with wings capable of changing their planes in consequence of the resistance of the air without being able to undergo helicoidal torsion. Finally, as the bird which flies with registering apparatus (Figures 32 and 35) has furnished an elliptical tracing and not an 8, as in the insect, you see, Sir, that the reply to your note to the Academy of Sciences is not easy, and this accounts for my taking the liberty of addressing you to explain the situation and to beg of you to let me know as soon as possible how I can give you satisfaction without entering into a discussion which the reader would have great difficulty in following. May I ask you to send me a copy of your learned Memoir in order that I may study it more at leisure, and profit in the future by your labours in a subject so interesting.—Receive, Sir, &c."

NOTE. In this letter it will be perceived Professor Marey asks me to limit my claim to certain points in order to avoid discussion. Having, however, limited or restricted my claim, Professor Marey insinuates in his reply to Professor Coughtrie that I am not entitled to claim priority in the figure-of-8 and wave movements without at the same time stating that he differs from me as to the manner in which these movements are produced. In other words, he wishes it to be understood that the original discovery of the figure-of-8 and wave movements are of little account in comparison with his explanation of the manner of their production. In reality, however, as Professor Coughtrie showed in his article, and as I pointed out in a letter addressed to Professor Marey in 1870, there is little real difference between us, and Professor Marey deceives himself and his readers when he states that his theory is wholly opposed to mine. The figure-of-8 as originally propounded by me includes, as I endeavoured to show in my letter of 1870, not only all the essential features of Professor Marey's hypothesis, but even its details.

I have described and represented the figure-of-8 made by the margins of the wing (figs. 2 and 3 of present communication); Professor Marey has described and represented the figure-of-8 made by the tip of the wing (fig. 5 of present communication). If, however, the margins of the wing make figures-of-8 during extension and flexion and during the down and up strokes, it is obvious that the tip of the wing, or any point between the margins, will also make a figure-of-8; further, that the arrows indicating these movements will point in opposite directions. There is no escaping from this conclusion. However incredible it may appear, Professor Marey in his letter to the French Academy in reply to my reclamation completely ignored these explanations, touched lightly upon the numerous points of resemblance, and emphasised the minor and supposed points of difference. He has wittingly or unwittingly persisted in making one half of my figure of 8 represent the whole, and has erroneously stated that in my figure of 8 the arrows all point in one direction; whereas in the completed figure they point in opposite directions as indicated at fig. 4 of present communication. My figure of 8 when completed, and it must be completed according to the text, is identical with that subsequently given by Professor Marey. I feel aggrieved at this treatment, as Professor Marey must be well aware that the part cannot in any case stand for the whole. With a perseverance and perversity worthy of a better cause Professor Marey even goes the length of stating that he has accurately counterdrawn what he designates my suppressed figure in the English edition of his "Animal Mechanism" (page 201, figure 86). It is quite true he has drawn and inverted my half figure, but, as Professor Coughtrie points out, he has in so doing totally misrepresented my views. One of two things is quite obvious, either Professor Marey does not even now understand the *rationale* of the figure of 8 as originally described and delineated by me, or (and I should be sorry to adopt this conclusion) he wilfully distorts and mixes up the details. It may be well to mention at this stage that while Professor Marey is aware that the wing of the insect makes a figure-of-8 track in space, he is even now ignorant of the exact course pursued by the wing. Thus he represents the wing as oscillating in a vertical direction, the down stroke according to him being delivered downwards and backwards, and the up stroke upwards and backwards as in fig. 5 of present communication. In reality, and as I long ago pointed out, the wing of the insect is made to vibrate in a very oblique and nearly horizontal direction, the down stroke being delivered not downwards and backwards but downwards and *forwards;* the up stroke being delivered not upwards and backwards but upwards and *forwards*, so (fig. 6 of present communication). This is a vital point. It is a physical impossibility for the wing to act as Professor Marey states. The arrows in Professor Marey's figure of 8 ought in reality to be reversed. To get a continuous series of figure-of-8 loops, or of forwards curves characteristic of progressive flight, the wing must, as I have all along maintained, *descend and ascend in a forward direction.* The tracings obtained by Professor Marey himself prove this conclusively; nevertheless he has failed to divine their precise meaning. That the true action of the wing is such as I indicate is placed beyond doubt by my own experiments with natural and artificial wings published in the "Transactions" of the Linnean and Royal Societies, and by recent experiments described in the "Ninth Annual Report of the Aëronautical Society of Great Britain."

(IV.) *Letter addressed by Professor Marey to the French Academy of Sciences in reply to a Reclamation lodged by Dr. Pettigrew* (*Comptes Rendus*, May 16, 1870).

"It is with regret that I have so long delayed the reply to the note of Mr. J. B. Pettigrew of date 18th April last. The author of that note reclaimed priority in the description of the figure-of-8 path pursued by the wing of the insect in flight. In support of his reclamation the author quoted several passages of a memoir which he has sent to the Academy. I have perused this memoir, and have ascertained that in point of fact Mr. Pettigrew had seen before me and represented in his memoir the figure-of-8 track made by the wing of the insect; that the optical method to which I had recourse is almost identical with his; but that we differ entirely in regard to the interpretation of the trajectory which we have both seen. Our disagreement refers to the direction of the movement of the wing during its course, the cause of its changes of plane, and the inflexions of its course which I ascribe to the resistance of the air. Finally, generalising his theory of the movements of the wing, the English author assigns to the wing of the bird the same trajectory as to that of the insect, whilst I have shown—in my lectures at the College of France, published last year in 'Revue des Cours Scientifiques'—that the wing of the bird moves according to a kind of ellipse of which the great axis is nearly vertical. In presence of these differences I have thought it right to address myself directly to Mr. Pettigrew, to explain to him the complexity of the dispute, and to ask him how I could reply to his 'Just Reclamation' without entering into a discussion which would unnecessarily complicate the question. It is only to-day that I have received the reply of the physiologist of Edinburgh. He holds to establish only 'that he has described and figured before me the figure-of-8 track made by the wing of the insect, and the spiral and undulatory curve which the wing of the insect, the bird, and the bat describes when these animals fly with a great horizontal velocity.' Whether this remark has been made by other naturalists nobody would venture to affirm, but in any case I hasten to satisfy this legitimate demand, and I leave entirely the priority over myself to Mr. Pettigrew relatively to the question so restricted. I hope to be able soon to submit to the Academy my experiments on the analysis of the movements of the bird during flight, in order that the Academy may judge the value of the processes which I have employed in that investigation."

NOTE. It appears to me that the real restrictive clause is that quoted by me, and not by Prof. Marey; the clause alluded to having reference to the figure-of-8, spiral, and undulatory movements originally discovered by me, rather than to Prof. Marey's interpretation of the manner in which those movements are produced. This follows because Prof. Marey, before replying to the Academy, and as the reader is now aware, requested me to limit or define the extent of my claim upon the movements in question: the restrictive clause certainly occurs in this connection. Prof. Marey, not content with mixing up two things essentially distinct, states that in my last work ("Animal Locomotion," 1873) I devote ten pages to impugning his theory of the flight of birds, and a special chapter to show to what extent my theory differs from his. These pages and that chapter, I may remark, were first published in 1870, and deal not with the figure-of-8 theory in its integrity, but with certain details which Prof. Marey has since modified. When I wrote them Prof. Marey (*vide* his earlier writings, "Revue des Cours Scientifiques de la France et de l'Etranger," March, 1869) maintained that the wing made *a backward angle of* 45° *with the horizon during its descent, and a forward angle of* 45° *during its ascent.* This view, shown on that occasion (1870) to be untenable, has been greatly modified in Prof. Marey's later works. Those of your readers who compare Prof. Marey's works with my own, and who have perused Prof. Coughtrie's article and examined the parallel passages and dates given in your Journal for April, 1875, will, I believe, have difficulty in accepting Prof. Marey's statement to the effect that "two authors could not easily have treated the same subject with more widely dissimilar methods, and arrived at more different conclusions." Prof. Marey makes light of the parallel passages and dates. He says Prof. Coughtrie "compares texts, and every time he meets with similar expressions in the English and French books he raises a cry of plagiarism, as if it were possible to treat of the flight of birds without speaking of wing, child's kite, inclined plane, sculling, &c." The question, however, has its serious aspect. The plagiarisms, if such they are to be designated, are not—as Prof. Marey would have your readers believe—confined to isolated words; they extend, according to Prof. Coughtrie, to clauses and passages, and these clauses and passages occupy many pages. It is easy to understand how two authors can write on the same subject, at the same time, and express the same ideas *in different language:* it is more difficult to comprehend how two authors can write on the same subject, at an interval of nearly two years, and express the same views in what many will regard as *virtually the same language.*

J. Bell Pettigrew, M.D., F.R.S.,

Professor of Medicine and Anatomy,
University of St. Andrews.

PROGRESS IN SCIENCE.

MICROSCOPY.—M. Ranvier communicates the following mode of preparing sections of bone to the "Archives de Physiologie":—A portion of the shaft of a long bone is procured, and immediately on its removal from the body is plunged into water, and allowed to macerate for the space of a year, the water being frequently changed. At the end of that time the bone will be found to have become as white as ivory, and quite free from any adhering tissue. The object of immediately plunging the bone in water is to prevent the infiltration of the canals and substance of the bone with fat. Sections are to be made with a saw, and these sections are ground down on pumice-stone, and finally polished on a harder material. In order to remove the powdered fragments of bone which have been ground off, and fill the canaliculi and lacunæ on the surface, it is sufficient to scrape the section with a scalpel. It is then placed in a warm solution of aniline dye, and allowed to remain there for two hours, and afterwards dried on a water-bath. The section is next rubbed on a hone, moistened with a 2 per cent solution of common salt. It is then washed in this solution, and permanently mounted in a mixture of equal parts of the solution of salt and glycerine.

Great attention has been paid of late to the staining of tissues with aniline and other dyes. Many structural details are no doubt rendered more distinct; but of course here, as in every kind of microscopical observation, care must be taken to avoid errors of interpretation.

It would appear, from enquiries in various journals, that many microscopists experience a difficulty in securing the cells of preparations mounted in glycerine. At the risk of repetition an effectual mode of preventing leakage is given, although, from the long time it has been in use, it might have been supposed that everyone had been acquainted with it. The object having been placed in the cell, and the cover adjusted, proceed to remove as much of the surplus glycerine as possible: if the quantity is large, as is sometimes the case with over-filled cells, most of it can be taken up by suction with a sharply-pointed pipette; the residue should be carefully absorbed with fragments of blotting-paper applied with the forceps; then, with a camel-hair brush and water, the cell and surface of the cover should be gently cleaned: care must be taken not to disturb the cover-glass. The varnish employed is the solution of shellac in wood-naphtha, known at the shops as "Liquid glue" or "Patent knotting varnish," and effectually resists the action of the almost universal solvent glycerine, if properly applied. The cell having been cleansed as much as possible from surplus glycerine, apply the varnish with a small camel-hair pencil, by hand, running it into the angular cavity at the junction of the cell and cover-glass, taking care, however, that only the smallest possible portion of cell and cover is included in this first coat. When dry, carefully wash the slide under a gentle stream of water; a tap partially turned on, or the ordinary wash-bottle, will answer the purpose. The object of this washing is to thoroughly cleanse the cell from adhering glycerine, which could not be removed while the cover was loose: dry the slide carefully by wiping with a cloth, taking care not to touch the cell and cover, which require a very tender drying with blotting-paper gently applied: the next coat of varnish is now to be laid on, and the turn-table can now be used to advantage, as the cover-glass is not likely to be disturbed. Do not at present coat the whole surface of the cell, but leave some part bare for the next varnishing. When dry, wash again as before, as the success of the whole process depends upon the freedom of the surfaces from any trace of glycerine. After this apply several coats of the varnish, taking care to keep each coat very thin. Never attempt to varnish thickly, as the interior seldom or never hardens. When sufficient varnish has accumulated, as shellac varnish becomes brittle after a time, add, for security, three or four coats of gold-size: this is the most tenacious and reliable varnish known, and will effectually protect the more brittle liquid glue, Gold-size is rapidly acted upon by glycerine, but when protected with a good coating of shellac is perfectly safe. Too much care cannot be taken in washing and cleaning the surfaces to be varnished from the least trace of adhering glycerine: the entire success of the process, as before mentioned, depends upon minute attention to this point, and if carefully carried out success is certain. The writer has objects mounted in glycerine fifteen years ago free from any trace of leakage. Liquid glue is also effectual in varnishing preparations mounted in castor oil: this useful medium has been much neglected, owing to the difficulty of preventing leakage. The cell and cover should be cleaned, as before mentioned, but, instead of water, benzole—carefully applied with a brush—must be used for the removal of the oil remaining after the application of the blotting-paper. The shellac varnish is to be followed by gold-size, as in the case of glycerine mounting.

A new method of measuring the absorption-bands in spectra has been communicated to the Royal Microscopical Society by Mr. H. C. Sorby. The apparatus consists of a crystal of quartz, cut so that the light passes along the line of the principal axis: the thickness used is exactly 1½ inches. This crystal is placed between two Nicol prisms, the upper one capable of rotation for the purpose of adjustment; the lower one fixed to an ivory circle, 2½ inches in diameter, each half of which is divided into ten parts, and these again into five smaller divisions, so that there is no difficulty in reading off to 1-100th of a half revolution. The light passing through this apparatus, when viewed with the spectroscope, divides the spectrum into eight spaces by seven well-defined bands, which in a prism spectrum are apparently at very uniform intervals, but at a much less wave-length interval at the blue than at the red end. When the apparatus is attached to the micro-spectroscope, the pointer of the circle being placed at zero, the upper prism is rotated so that the centre of the second dark band from the red end of the spectrum exactly coincides with the sodium line, or with the solar line D. All the other dark bands are then in perfectly constant and definite positions, depending on the action of quartz on light of various wave-length. On rotating the ivory circle each band gradually passes from the red end towards the blue, until when the circle comes to the next zero point, commencing the next semicircle, the series of bands is exactly the same as at first. The formula for reducing the degrees of the scale to their respective wave-lengths is given, and also a table for an instrument constructed with a crystal of quartz of exactly 1½ inches in thickness, cut and mounted as described. The chief objections to the apparatus appear to be the difficulty of obtaining and cutting a suitable piece of quartz, and also the size of the apparatus, its length being about 3½ inches, which renders the instrument inconvenient for attachment to the ordinary micro-spectroscope, but it has answered well placed below the stage of Mr. Sorby's binocular spectrum microscope.

The Quekett Microscopical Club have issued a Catalogue of the preparations in their cabinet, containing the total number of 2041 slides. The subjects most largely represented are—Hairs of animals, of which there is a collection of 238 specimens, including a very complete series from Indian and other bats; the seeds amount to 217—these and the greater part of the hairs, especially those from India, were the gift of Dr. M. C. Cooke, as also most of the microscopic fungi, of which there are 69 specimens; the ferns are well represented by 171 slides. The objects can be borrowed for examination by members, under certain conditions, offering great facilities to those who wish to compare at their leisure a series of specimens. The whole collection has been formed by the liberal donations of members of the Club, and it is to be hoped that the annual increase may be a large one, which will certainly be the case if mounters of objects will send their duplicate specimens to the cabinet.

An ingenious little instrument, for readily cleaning very thin covering-glass, has been contrived by Mr. W. W. Jones, of the Quekett Microscopical Club. It consists of a small tube of brass or steel, of about an inch in diameter and the same in height, into which fits loosely a weighted plug. To the lower end of this plug is cemented a piece of chamois leather. Another piece of leather is stretched upon a flat piece of wood or plate-glass, to form a pad, which completes the apparatus. In using, the tube is placed on the pad, and the thin glass, after breathing on, dropped in; the plug is then inserted, and, holding the tube well down on the pad, any amount of rubbing can be used with perfect safety, the weight of the plug giving sufficient pressure.

The subject of double staining of wood and other vegetable tissues has been experimented upon by Dr. George D. Beatty, of Baltimore: the results will be found useful to those interested in preparing tinted sections. The author has discovered that benzole instantly fixes any aniline colour in vegetable tissues, and also renders them as transparent as oil of cloves. The following are the processes adopted:—A section of wood or other vegetable substance, being prepared for dyeing, is put for five or ten minutes in an alcoholic solution of *roseine pure* (magenta), one-eighth or one-quarter grain to the ounce. From this it is removed to a solution of "Nicholson's Soluble Blue Pure," one-half grain to the ounce of alcohol acidulated with one drop of nitric acid. In this it should be kept for thirty or ninety seconds, rarely longer. It should be frequently removed with forceps during this period, and held to the light for examination, so that the moment for final removal and putting into benzole be not missed. After a little practice the eye will accurately determine the time for removal. Before placing the object in benzole it is well to hold it in the forceps for a few seconds, letting the end touch some clean surface, that the dye may drip off, and the object become partially dry. By doing this fewer particles of insoluble dye rise to the surface of the benzole in which the brushing is done to remove foreign matter. The object should then be put into clean benzole. In this it may be examined under the glass. If it is found that it has been kept in the blue too short a time it should be thoroughly dried, and after dipping in alcohol be returned to the dye. If a section of leaf or other soft tissue be under treatment it should be put in turpentine or *oil of juniper*, as they do not cause so much contraction as benzole. When hæmatoxylin is used instead of magenta, it is followed by the blue as just described. As neither of these dyes come out in alcohol or oil of cloves, the section may be kept in the former for a short time before placing in the latter. The hæmatoxylin dye preferred by Dr. Beatty is prepared by triturating in a mortar for about ten minutes two drachms of ground campeachy wood with one ounce of absolute alcohol, setting it aside for twelve hours, well covered, triturating again and filtering. Ten drops of this are added to forty drops of a solution of alum; twenty grains to the ounce of water. After an hour the solution is filtered. Into this the section, previously soaked in alum water, is placed for two or three hours, or until dyed of a moderately dark shade. When dyed of the depth of shade desired, which is determined by dipping it in alum water, the section is successively washed for a few minutes each, in alum water, pure water, and 50 per cent alcohol. Finally it is put in absolute alcohol until transferred to the blue. Carmine and aniline blue produce marked stainings, but they are rather glaring to the eye under the microscope. An ammoniacal solution of carmine is used, double the strength of Beale's, substituting water for a glycerine. In this a section is kept for several hours. On removal it should be dipped in water, and then put for a few minutes in alcohol acidulated with 2 per cent of nitric acid; then in pure alcohol; then in the half-grain blue solution before spoken of, from which it should be removed to alcohol; then to oil of cloves. Much colour will be lost in the acid alcohol. The acid is to neutralise the ammonia which is inimical to aniline blue. Magenta or hæmatoxylin may be used with green instead of blue aniline. Iodine green is to be preferred, one grain to the ounce of alcohol. Double stainings of leaves in which red is first used have the spiral vessels stained this colour, other parts being purple or blue. Radial and tangential sections of wood have the longitudinal woody fibres red, and other parts purple or blue. This selection of colour is supposed by Dr. Beatty to be due to the fact that spiral vessels and woody fibres take up more red than other parts, and are slower in parting with it. The blue, therefore, seems first to overcome the red in parts where there is less of it. It will entirely overcome the red if sufficient time be given. If the blue be used before the magenta aniline, the selection of colour is reversed. The sections should be mounted in Canada balsam, softened with benzole, as the presence of the latter may be beneficial in preserving the magenta. Respecting section-cutting and preparing sections for dyeing.—To cut a thick leaf, place a bit of it between two pieces of potato or turnip, and tie with a string. Cuts may be made along the midrib, or across it, including a portion of leaf on either side, or through several veins. Fine shavings of wood may be used, or pieces rubbed down on bones. Sections of leaves may be decoloured for staining by placing for some time in alcohol; but Labarraque's solution of chlorinated soda is to be recommended for twelve or twenty hours after the alcohol. In twelve hours wood is generally bleached; too long treatment will, however, cause it to fall in pieces. After removing from the bleaching solution wash through a period of twelve or eighteen hours in half a dozen waters, the third of which may be acidulated with about ten drops of nitric acid to the ounce, which acid must be washed out. Next put in alcohol, in which sections and also leaves may be kept indefinitely ready for dyeing. One week instead of forty-eight hours is frequently required to effect the decolouration of large leaves in chlorinated soda, even when they are cut into several pieces, which is advisable. Mr. L. R. Peet, of Baltimore, thinks better results are attained by commencing with a weak dye, say from one-twentieth to one-twelfth of a grain, and slowly increasing the strength of the dye at intervals of from one to three bours until the required hue is obtained. This process guards against too deep staining, and gives a finer tone to the leaves under the microscope.

ENGINEERING, CIVIL AND MECHANICAL.—*Bombay Docks.*—The question of providing suitable wet docks for the Port of Bombay has for many years been under consideration by the Government in India. Perhaps one cause which has contributed, as much as anything else, to delay commencement on the work, has been the fact that two rival schemes have been in existence, one of which had for its object the location of the Docks on the Elphinstone Estate, and the other at Moody Bay. It having, however, now been determined that the advantages of the former site have the pre-eminence over those of the latter, it has been determined to construct the Docks there from the designs of Mr. Thomas Ormiston, the Engineer to the Bombay Port Trust. Contracts for the masonry part of the work have been let to the firm of Messrs. Glover and Co., Contractors, of Bombay. The first stone of the Docks was laid by the Prince of Wales on the occasion of his visit to Bombay. Further particulars of this work will appear in some future number of the "Quarterly Journal of Science."

Liverpool Landing-Stage,—It will be remembered that on the afternoon of the 28th of July, 1873, the great landing-stage at Liverpool was destroyed by fire, an occurrence which caused no little inconvenience to the passengers crossing the Mersey. The old stage consisted of iron pontoons, which supported five large wrought-iron kelsons on box girders, about 20 feet apart, running longitudinally the whole length of the stage. Across these kelsons were placed pine beams the width of the stage, and varying in thickness from 16 inches to 14 inches by 12 inches. Upon these beams was fastened the longitudinal pine deck, or planking, 6 inches by 4 inches, and crossing this again were greenheart sheathing planks, 6 inches by 2 inches. The whole of this was caulked and pitched, to make it independent of the action of the water and the weather. The new stage consists of the pontoons and large wrought-iron girders as before, but, in place of wooden beams, iron beams have been substituted through the entire length of the stage. The pine deck-planking has been replaced with greenheart, and greenheart sheathing, as before, completes the decks. The new wrought-iron deck-beams weigh nearly 1500 tons, and this fact alone will give some idea of the magnitude of the undertaking, which was successfully completed by Messrs. Brassey, who were the contractors for the work.

A paper has recently been read before the American Society of Civil Engineers, on "The Water Front of New York," which furnishes some very interesting particulars of the engineering works undertaken for the accommodation of the trade of that port. The extent of available accommodation for shipping is shown by the fact that the city has 25 miles of water front within the limits of the island, and this has lately been largely increased by the acquisition of territory north of the Harlem River, all of which is available for quay and wharf purposes. The upper bay includes within its area 13 square miles of safe anchorage for large vessels, and the lower bay about 88 square miles. The average rise and fall of its tides is less than 5 feet. The lower part of the island is generally formed of sand and gravel overlying granitic rock. The upper portion, which is high on the west side, is generally rocky, the rock being granite, gneiss, mica-schist, and blue bolders, of a quality fit only for ordinary foundations. A considerable depth of black and blue mud overlies the sand, gravel, or rock of its shores. The earliest record connected with wharves and docks in the city is one dated 1654, when Daniel Litchoe, tavern-keeper, was authorised to build a dock on the strand; and in 1660 the burgomasters received permission from the Director-General to take a certain sum from shippers and owners for the erection of a pier for the accommodation of the inhabitants. The growth of the wharfage extent was gradual up to the commencement of the present century. The form of the construction of the early wharves, where hard bottom could be reached at ordinary depths, consisted in alternate cribs of wood filled with stone, and bridgeways of from 10 to 20 feet span. When holding-ground for piles could be found, piles were in many instances used. The retaining walls, or bulk-heads, were constructed of cribs, as now, but the carpentry was of the rudest kind. In 1867, the value of existing wharves, piers, and slips owned by the city of New York was estimated at nearly 16 millions of dollars. In 1870 a Department of Docks was established, who initiated a plan of permanent improvement of the water front of the city. For the new river wall, the general system adopted in 1871 was the same as that established so far back as 1782, namely, a system of narrow wharves and slips, affording the longest wharf and quay-line for the shortest extent of water frontage, combined with readiness of access. The river wall is composed of *beton* blocks weighing from 25 to 50 tons each, extending from the foundation to within about 2 feet of low water mark, and, above this level, of concrete laid in mass faced with ashlar granite masonry. The blocks are composed of—by volume—1 part of Portland cement, 2 of sand, and 5 of stone broken to pass through a 2-inch ring. These proportions have lately been changed to 1 part of cement, 2½ of sand, and 6 of broken stone. When concrete is laid loose under water, the proportions are changed respectively to 1, 2, and 3 or 4 parts. The wharves are all constructed of wood. The pier-heads are the only novel features in these structures; they are constructed of built-up columns, 20 inches by 20 inches in section, and 75 feet in length, placed in rows 12½ feet apart and 9½ feet apart in the rows. The rows are sheathed from low water up to the girders on both sides with 5-inch planking, the ends of which are protected with boiler plates. The heads of the columns are securely framed into the caps and girders. The piles used in the pier, some of which are 95 feet long, are driven in rows 8 feet apart, 5 feet apart in the row, and securely braced. The square timber is 12 inches by 12 inches in section. The columns are likewise braced with 1½-inch rods, extending from the bottom to low water.

A new addition has, within the past few years, been made to the list of Scientific Societies by the establishment of the "Association of Municipal and Sanitary Engineers and Surveyors." The inaugural meeting of this association was held in May 1873, at the Institution of Civil Engineers. The Association has since held meetings in various provincial towns, and several very interesting papers relating to drainage, sewerage, ventilation of sewers, the treatment and disposal of sewage, water supply, and cognate subjects have been contributed by its members. We shall watch with much interest the progress of this young society, and shall, on a future occasion, refer more particularly to the papers read at its meetings.

Mr. Hugh Leonard, M.I.C.E., Chief Engineer under the Government of India, has published in "Engineering" a memorandum drawn up by him on "The Weight on Foundations of Buildings." Mr. Leonard was led to enquire into this subject when commencing several new buildings in Calcutta, several buildings of recent construction being cracked, and some of them badly. The conclusions drawn, after a long series of experiments, were as follows:—"If it be desired to provide against sinking of the whole structure, the weight per foot on the earth, whatever may be the depth at which the foundation is laid, should not be more than one ton per square foot; and that, if unequal sinking cracks are to be provided against, the weight on all parts of the foundations should be equal per square foot, unless that the most heavily-loaded portion carries less than one ton per square foot. With this weight, and, of course, with lighter weights, no perceptible compression takes place, and hence no unequal settlement would occur from unequal loading." From experiments relative to the best depth for the foundation, it was ascertained that shallow foundations have the disadvantage of being affected by climatic influence, as heavy rain caused the masonry laid at a depth of 2 feet 6 inches below the surface to sink considerably. "Again it appears that foundations laid at a depth of 11 feet below the surface sink more than those laid at depths of 4 feet and 8 feet." The conclusions drawn from these experiments were that "the foundations of important buildings should be laid so deep that they cannot be affected by climate," and that they "should not be laid at a less depth than 4 feet, nor a greater depth than 6 feet." The last series of experiments undertaken were to ascertain the strength of spread foundations, and the conclusions drawn from them were that "for a pressure of one ton to the foot, on Bengal soil, the thickness at the toe of the slope should not be less than 1 foot 6 inches, and the stepping at an angle of not more than 45°.

A new market-place has recently been opened in Madrid, the materials of which were all sent out from this country. In design these markets are somewhat similar to the Halles Centrales in Paris, but are bolder, more ornamental in character, and very lofty. Structurally, the markets are composed of separate pavilions, the Mostenses Market having three, each 127 feet long and 90 feet wide, the pavilions being connected by passages, making altogether a rectangular area of 38,500 square feet. The Cebada Market covers an irregularly shaped area of 60,000 square feet, and is composed of four principal pavilions, each 119 feet long and 79 feet wide, three irregular pavilions and one lofty central dome, the pavilions being connected by covered ways, as at Mostenses. In these two structures there is nearly 4000 tons of cast- and wrought-iron. Below each market the space is utilised for cellars, the ground-floor being supported by cast-iron stanchions. The roofs are covered with galvanised corrugated iron, and there is ample ventilation by open louvres in the roofs and sides of each pavilion.

ELECTRICITY.—The following process has been recently invented in France by M. Hausen, for depositing metallic coatings upon glass or porcelain:—Sulphur is dissolved in oil and lavender and evaporated to a syrup; chloride of gold or of platinum is also dissolved in ether. The two solutions are mixed and slightly heated. They are finally evaporated to the consistence of ordinary oil-paint, and applied with a pencil to those parts of the glass or porcelain upon which it is desired to deposit the metal by the battery.

An apparatus consisting of two movable parts has been constructed by MM. Terquem and Trunnin for perforating glass with the electric spark. In the upper part of the apparatus a vertical brass rod, with ball above, point below, is enclosed in two concentric glass tubes, the small intervals being filled with colophonium. The brass point reaches through to the lower side of a horizontal glass plate attached to the tubes. In the lower part of the apparatus a right-angled brass rod is fixed, also in colophonium within a glass tube, its vertical part passing up the middle, and terminating in a point on the upper side of a horizontal glass plate resting on the tube. The tube is supported on porcelain and wood. The plate to be perforated is first coated with oil. and put between the two horizontal plates of the apparatus. The metallic points are brought opposite each other, and the outer ends of the rods put in connection with an electric machine or induction-coil.

Professor W. L. Brown, of the University of Georgia, calls attention to a remarkable instance of the formation of impressions upon the human body by a lightning stroke. On the 12th of July, 1875, at about 4 p.m., a stroke of lightning fell upon a house in Americus, Ga., rendering insensible for a time four persons who were seated in one of the rooms. The two outer sides of this room, which was at a corner of the house, had each one window, and nearly opposite these on the two other sides were the chimney and the door respectively. Outside, a tree stood in front of each of the windows, and about 12 feet from the house. A third tree, a locust, stood opposite the outer corner of the room and about the same distance from the house as the others. This tree was severed by the lightning, but the other two were not affected. A young child was sitting near the centre of the room, while the mother and a lady were seated not far from the chimney, near which, and close to the wall, was another child. All these persons were rendered insensible for a time by the stroke which severed the tree, and on their recovery there were found impressed upon the bodies of them all more or less distinct images of this tree. The child near the centre of the room was impressed upon its back and exactly opposite upon its stomach. The entire tree was plain, and perfect *in toto*; every limb, branch, and leaf, and even the severed part, was plainly perceptible. It impressed the young lady upon the left hip and right leg, the mark being quite as perfect as that upon the body of the child. The mother and other child also bore less distinct impressions upon the leg. The marks are not permanent, for on August 7, the impressions were no longer distinct. It is possible to produce similar figures artificially with an electrical machine, such as the Holtz machine, capable of giving electricity of very high potential. When the poles of the latter are strongly charged and are separated to the distance of a few inches, the discharge, instead of producing a spark or brush, sometimes consists of a very small jet upon the negative, and a sort of phosphorescent glow upon the positive. The space between them, though not luminous, is the seat of a discharging action which appears to take place along definite lines, like a stream or current, and is sometimes called the dark discharge. An object placed between the poles, and in the path of the discharge, interrupts this, and destroys the glow upon the positive pole in points corresponding to the lines thus broken; and in this way there is produced an image or shadow of the interposed object, which is often strikingly distinct and perfect. In the case above described the phenomena are readily accounted for, if we suppose the thunder-cloud to have been negatively charged, and the tree to have stood in the path of the dark discharge which preceded or accompanied the lightning stroke, the action having been sufficiently intense, and the quantity of electricity great enough, to produce a visible impression upon the delicate tissues of the skin.

The *Oestereichische Landwirthschaftliche Wochenblätter* states that Dr. Virson, Superintendent of the Italian Experimental Silk-Farm at Padua, has discovered that the hatching of silk-worm eggs, of suitable age, may be accelerated by a period of ten or twelve days, and a yield of at least 40 per cent of silk-worm caterpillars secured, by exposing the eggs to a current of negative electricity from a Holtz machine for the space of eight or ten minutes. It is suggested that the same method might perhaps prove useful in hastening the germination of various seeds.

In a recent number of *Les Mondes* the Abbé Moigno directs attention to hygienic application of electricity. From his remarks it would appear that Dr. Poggioli has advised a system of "electrical gymnastics." The exact nature of these "gymnastics" is not stated, but it is mentioned that recent trials, in the presence of a committee appointed by the Prefect of the Seine, upon twenty-one school children of known physical weakness and mental debility, resulted in improved respiration and appetite, as well as in improved mental conceptions and an increase in height, weight, and chest-measurement. These beneficial effects are said to have remained three months after the conclusion of the course. *Les Mondes* also states that the same Dr. Poggioli has succeeded in proving that electricity may be usefully employed as a sedative in nervous affections and certain acute forms of disease.

TECHNOLOGY.—In consequence of the low pressure of gas during the day-time, trouble is often experienced from the retreating Bunsen burners of the usual construction. This having repeatedly proved a source of annoyance and loss, President Henry Morton, of the Stevens Institute of Technology, was led to the following consideration of the subject:—The retreat of a burner will evidently occur whenever any part of the ascending column of mixed gas and air is moving at the orifice with a velocity less than that at which the same will burn downwards. In an ordinary burner, with its main tube of regular cylindrical bore, it is evident that the friction of the surface of the ascending column of mixed gases will cause that portion to move at a less velocity than the central part, and that currents of the nature of eddies will be developed. It will thus happen that while the central portion of the ascending column of gaseous mixture issues at a velocity much greater than that at which the material can burn downwards, and thus is quite free from any danger of retreating, the marginal portions of the column or jet of gas will be escaping at a rate so much less that the velocity of their combustion downwards will exceed that of their upward motion, and retreat of the flame will ensue. It is well known that to secure a jet of water, or of any other fluid whose particles shall move with equal velocities in all parts, and thus avoid currents and eddies, it is only necessary to make the orifice of efflux an aperture in a thin wall. Following out the idea above indicated, President Morton made a burner of a bore rather large compared with its height, and then drew in its upper edge into the form of an open-ended thimble, so contracting the orifice of escape to about two-thirds the area of the tube, and rendering this orifice practically an opening in a thin horizontal wall or plate. The results of this modification far surpassed his anticipations. A burner thus constructed gives a perfectly non-luminous flame with gas pressures varying between 1·5 and 0·1 inch of water, and with the lowest of these pressures cannot be made to retreat by the most violent handling in the way of sudden movement or waving about in the air, even when this violence is carried to the extent of extinguishing the flame altogether. Under like conditions of pressure, a burner of the ordinary construction is made to retreat by a slight draught of air, or a very moderate amount of motion.